

OBITUARY NOTICE

Dr JOHN CARL FLUGEL

John Carl Flugel was born in London in 1884 and died there on 17 August 1955. He leaves a wife, daughter (Mrs Hugo Harper) and grandson (Nicholas).

He was successively Demonstrator in Psychology, Senior Lecturer, Assistant Professor, and, after his retirement, Special Lecturer at University College, London, serving the Department with devoted faithfulness until the day of his death. The British Psychological Society owes much to his support as Honorary Secretary, Honorary Librarian, President and, latterly, Chairman of the Social Psychology Section. With Dr C. S. Myers, he was largely responsible for the changes by which the Society, from being a small, select body of professionals, became a large and still growing association of generally interested people. In 1950 he was President of the Psychology Section of the British Association for the Advancement of Science.

He was privately educated until he went up to Oxford, with an exhibition, at the early age of seventeen, to read Classical Moderations and 'Greats'. At once he showed independence by attending Prof. Gotch's lectures on physiology, studying experimental psychology under Dr William McDougall; hypnotism in a practical way, and joining the Society for Psychical Research.

Though many English people could have easily recognized that Flugel had been to Oxford, he had none of the personality signs which, rightly or wrongly, are attributed to the Balliol man of his time. He widened his interests and experience further by studying experimental psychology at Würzburg under Prof. Oswald Külpe. His sympathetic—may one also suggest empathetic?—account of the Würzburgers' 'Denkpsychologie' may owe much to the fact that he understood its attractions and defects from the inside.

An early research, carried out with Cyril Burt, employed intelligence tests in a way unusual at that time, displaying the social interests of these young psychologists by comparing the performances by the boys of a Liverpool elementary school and a well-known Oxford school; many of its pupils sons of intellectually distinguished or socially successful parents.

He was such a prolific writer that even to list his publications, over eighty, would require the space allotted to this article. It may afford some insight into his many-sided nature and his rich culture (he could be informative and witty in five European languages and informative in Esperanto) to mention some outstanding books and to comment on the interests and motives behind their production.

Understandably enough, his first book, *The Psychoanalytical Study of the Family*, reflects Freud's early views more directly—perhaps more obediently—than Flugel's latest books did. There had at that time been little co-operation between psychologists and anthropologists, who had only just began to report facts which seriously complicate any simple picture of loves, hates and tensions within a family. The developing concepts of 'culture-pattern', with the reminder 'Some do and some don't', were hardly above the horizon.

He was now busy with laboratory experiments and discussed some of his researches in *Practice, Fatigue and Oscillation*, developing concepts for which the University College, London, laboratory became well known. His years of work in conducting and directing research proved that though a convinced and practising psycho-analyst, he was keenly interested in scientific experiment in the strict and narrow sense (at that time) of the term. His example to students, and his enthusiasm for both these branches of study, were of the greatest service to Prof. C. E. Spearman, whose own interests were developing in other directions. Flugel was one of the few British psychologists to be simultaneously interested in laboratory experiment, factor-analysis and psycho-analysis.* While he did much more than his contemporaries to dovetail these three branches of study, it cannot be said that they have yet been really amalgamated.

His generous encouragement of students, especially perhaps of the foreign, lonely and troubled ones, went much further than an interest in laboratory working hours. At his house in Regent's Park, and later in Hampstead, he and his wife, who herself is deeply interested in psychology and wrote several articles for the journals, welcomed hundreds of guests, lecturers and students, and made them feel at home, usually by talking to them in their own language. Any distinguished psychologist who visited London to lecture was sure to be entertained in some way by the Flugels, and to meet people, specially invited, conversant with his special subject.

Either deliberately or for temperamental reasons, Flugel was apt to break, or cheerfully to ignore, taboos. Unlike some contemporaries, he did not defy a convention for fun or to court publicity. More often it was done after serious consideration, and quietly, to save the feelings of as many persons as possible. Dazzling the bourgeois would have given him no thrill; he was one himself, and probably proud of it. Social differences were for him part of the general English scene; he took them for granted. There are plenty of well-trodden paths—perhaps even a few tramlines—in psychology to-day, but Flugel preferred to climb a cliff; usually alone. His *Psychology of Clothes* strides cheerfully over several *Verboten* signs; the subject is hardly mentioned to-day by social psychologists, its discussion being discreetly left to the anthropologists, with their special dispensation. This book is one of the most interesting that he wrote. Though in it speculation is at times difficult to distinguish from fact, the argument cannot fail to interest any thinker unafraid of appearing unacademic.

While he was never an anti-intellectual or an irrationalist in the sinister senses of these terms (indeed, his sturdy and constant support of the South Place Ethical Society, and his many addresses at Conway Hall are cogent evidence to the contrary), he never accepted a neat division between the 'faculties' of Reason and Emotion; a subject about which so many unconvincing books have been written. Yet, Londoner and traveller that he was, he was deeply impressed by the role of symbolism in our world to-day, with its national flags, swastikas, hammers and sickles, animal totems, religious symbols, titles and prestige-garments. At times his suggestions that *X* might be a symbol of *Y* provoked attacks from literary people who would rush to defend symbolism in poetry, drama or visual art. It is, however, difficult to counter the common man's objection; '*X* may have made Freud think of *Y*; that was his private affair, not mine', on the level of common sense. But, like Freud, Flugel was fond of speculating—as, indeed, some historians are—about

* Dr Ernest Jones has described Flugel's relations with and contributions to the psychoanalytic movement in an obituary article in the *International Journal of Psychoanalysis*, 37, part 2. (1956).

the motives, conscious or unconscious, of famous people long dead, who can offer no relevant evidence.

A Hundred Years of Psychology has been a classic for years. It is the standby of lecturer and student, to be studied with notebook and pen, yet a joy to read. Erudite, carefully documented, exact, fair, it never displays that frozen attitude often praised as detachment. In each chapter he enters into the spirit of psychologists of the period. To use William James's phrase with a different reference, here was a psychologist incapable of being bored.

In contrast to this selfless, respectful presentation of the thoughts of others, *Man, Morals and Society* gives us Flugel's own views. As an undergraduate, he must have attended many lectures by moral philosophers, but some of these had not yet heard of, or did not wish their circles to be disturbed by intruding psychologists. Yet Flugel felt that the attitude of a single person, however obscure or humble, towards an act which he is about to perform, or to refrain from performing, is important, whatever the totalitarians of administration or theoretical philosophy may hold. As he wrote elsewhere (*Psychological Factors of Peace and War*) in urging more efforts for world integration:

...it will be well as often as possible to supplement the cold abstract notion of the state with the warm concrete images of individuals, not, of course, the 'stereotyped' individuals, corresponding to the traditional notion of the Englishman, German, Frenchman, Russian, Japanese, etc, but real men and women in all their rich humanity and variety.... The more we are made to think of what war means to individuals, the blasting of their bodies, the blighting of their hopes and the utter disorganization of their plans, the pathetic destruction of the little things they loved and used—all of them matters which we ourselves can understand from our own personal experience—the more will the monstrous moral iniquity of war be made manifest to us.

He differed from many writers about war and peace in that his detestation of war was combined with an extraordinary insight into its present-day causes. The whiffs of opium; 'Nobody wants war' and 'Wars are due to fear', had no effect on him. He calmly listed the social forces which to many people make peace appear dull, and war, especially in prospect, attractive. They make uncomfortable reading to-day.

If it is fair to charge social psychologists with neglecting 'non-psychological' causes of war, Flugel was certainly exempt. He wrote *Population, Psychology and Peace*; on over-population as a cause of wars, and in his writings showed very clearly the difficulties in the way of instilling supranational loyalties, not disguising his opinion that few people are trying hard to do this.

In the best sense of the word, he was an academic. If he was chairman of a meeting, it would be conducted as one expects in a seat of learning. He would listen to the speaker with unfeigned interest, and make opening comments invariably related to the paper which had been contributed. Never pedantic, he wore his learning lightly; in the best tradition, he often appeared to be merely reminding the audience of something they knew already, but might temporarily have forgotten.

Why social psychologists so often exclude from their writings the discussion of humour and wit (laughter is more respectable) is a problem far from insoluble. Flugel's long, richly annotated chapter 'Humour and Laughter' in the new *Handbook of Social Psychology* (edited by Gardner Lindzey) reminds us that though humour has a variety of meanings, they are all derived from the original sense of 'moisture, fluid'. And, whatever he wrote, it was never dry.

Though he might have been startled at the appellation, he was a gosseller, on the subjects of friendship, mutual understanding and world peace. A recent broadcast was on Tolerance. Probably the last of his public services was to give an address on 'The implications of psychology in modern social life', an excellent account of what psychologists today are trying, and not trying, to do. The audience was chiefly composed of people from other countries.

He never behaved like an elder statesman expecting youth to show deference, though he punctiliously accorded it to his elders. He was always young in mind, and the photograph represents him as he will be remembered by his older friends.

Bertrand Russell has written 'We must cleanse our hearts of the poisons that make war seem reasonable: pride, fear, greed, envy and contempt.' Jack had done this before I first met him. And that was forty years ago.

T. H. PEAR

METHODOLOGY OF TRAIT ANALYSIS

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It is proposed that traits, as assessed by persons (X 's) about their fellows (Y 's), are best explained from a self-theoretical standpoint, and not, as hitherto, from a so-called 'objective' framework. Theoretically, modes of regard of the X 's (not to be confused with 'halo' effects) are at issue, and not elements of temperament of the Y 's. Modes of regard can be represented by factors, attributed to the X 's and not to the Y 's. Methodological matters are discussed which support this new look at traits and their analysis.

I. THE PROBLEM

Studies of personality traits by Cattell (1945, 1946*a*, *b*, 1947, 1948), Burt (1946, 1948), Eysenck (1953) and others are pursued in a highly systematic manner, with the best technical resources, so that they appear to be on the right lines scientifically. Clinically orientated psychologists, however, do not find that these studies help them to understand personality any better (Albino, 1953; Uytman, 1954), and it is easy to suggest that the factorists have a great deal of science and many operations, but no theory, whereas the clinical psychologists have things the other way around. The problem, perhaps, is to provide the former with a theory or theories. I propose, therefore, to subject a paper by Cattell, as representative of these factor studies, to a methodological examination, and to provide it with the appropriate kind of theoretical framework. The paper under consideration (Cattell, 1947) is one in which the author analyses thirty-six personality traits which are central to his 'description and measurement of personality' (Cattell, 1946*a*). The data (Reyburn & Raath, 1949) have been shown to be highly consistent (Banks, 1948), although only in some respects (Reyburn & Raath, 1949), and the facts appear to fit well not only with Cattell's own views, but also with those of Eysenck (1953) and of Burt (1946) as a re-factoring by Banks (1948) has indicated. There is little reason to doubt that the facts might fit still other theories. The thirty-six traits have a comprehensive coverage of the literature, and it has somehow seemed important to study their kind, to judge by the attention given to them by Thurstone (1947*b*), Guilford & Guilford (1939) and others.

II. METHODOLOGICAL CONSIDERATIONS

The concern in Cattell's study was with students (X 's), who assess their fellows (Y 's) for the temperamental traits of the latter. The traits were of the kind *cheerful*, *energetic*, *sociable*. The conditions under which the ratings were undertaken were carefully prescribed, as follows:

The proper planning of rating... requires such essentials as that the raters live with the ratees; have no official relation to them; are not asked to rate more than about twenty people, because more cannot be known well; deal with all subjects at one time with respect to one trait and not all traits with respect to one subject; have traits behaviourally defined and illustrated; have the ratings pooled for at least ten judges to cancel individual perspectives (Cattell, 1947, p. 198).

Under such conditions, Cattell proposes, the 'only remaining source of systematic error is halo or stereotype', which, he adds, has never been demonstrated in trained raters. Data

gathered under these conditions were correlated and factored. The factors are thereupon referred to the Y 's, the subjects under observation, by the X 's. This, I propose, is an operational mistake. The operations are not those of the Y -population, but of the X 's. One should ask, therefore, what might be expected when relatively naïve subjects, such as the X 's, observe their fellows in terms of designated traits? It is possible that everything might be explained, theoretically, from the frame of reference of the X 's. That is, there might be a theory which is intrinsic to the X 's, and this apart from halo effects, or from any arbitrariness on the part of X 's. The concern might be less with the supposed personalities of the Y 's and more with the way we acquire and use traits; it may be wondered, for example, how naïve subjects, such as the X 's, are able to give meaning to complex traits such as *punctilious* or *hypochondriacal* or *unscrupulous*. Little seems to be known about the development of language in relation to human behaviour, and a theory for such matters would perhaps be of some general interest.

A second gap in Cattell's study stems from two related sources, first from the lack of any deductive possibilities for the factors he reaches, and secondly, from restrictions imposed upon his data arbitrarily by the *simple structure* solution to the rotation of his centroid factors. As for the first: Cattell distinguishes between *principal surface* and *principal source* (or primary) traits, the latter being elements of which the former are composed. Given a surface trait it is always possible to analyse it into its constituent primary or source traits. But, given the primaries, Cattell does not deduce from them what surface traits are possible. The situation is like that for flagstones; given a pavement one can always distinguish the primary flagstones, but given the flagstones, what can one make with them? Clearly a great deal, and much besides flagstones. Cattell's system works only one way, down into primaries, but not up again into complexes.

As for *simple structure*: this, as is well known, looks for a solution to the rotations, such that no variate is on more than one factor. In Cattell's study twelve centroid factors are rotated to reach twelve primaries (A to L) in simple structure. There are no traits, if the solution is reached, compounded of two or more primaries; certainly such composites are minimal, for it has been usual in the past to reject tests for which primary factors cannot be reached, on the ground that they are impure mixes of other, well-established, primaries. French (1949) has noted that tests of scholastic attainment are impure in this way. However, it is scarcely possible to discard *traits* from a matrix because they are impure mixes of primaries. Equally, it is scarcely adequate to regard complex traits, such as Cattell's principal surface traits, as mere mixes of primaries, in the sense that so much white and so much black make grey. The essential problem is to *explain* the complex traits in terms of some primaries. If this could be done, then a remedy would be at hand for the weakness just noted, that Cattell's system works down into primaries but not up again into complexes. Before developing such an explanatory system, however, it is worth noting that Cattell's search for simple structure may have resulted in an analysis into too many unrelated, and *unrelatable*, primaries. There is a possibility that data might be analyzed too far, into fragments rather than into properly analytical primaries. Banks (1948), it will be recalled, required only four or five of Cattell's twelve centroid factors in order to explain Cattell's thirty-six traits to her satisfaction. The other centroids could be regarded as either insignificant, or as minor and unimportant complications of the main factors. It is possible, likewise, that four primaries, *plus the composites they generate*, may have advantages for explanation which are lost when the same matrix of variates is reduced to

twelve primaries in simple structure. In short, *simplest* structure (as I have elsewhere called it (Stephenson, 1953)) may be the key to the analysis of Cattell's traits, rather than analysis into too many factors in *simple* structure.

III. EXPLICIT AND REDUCTION DEFINITIONS

The problem is to explain complex traits in terms of relatively few primaries. Clearly, much depends upon what is to be meant by an explanation. Factorists have rarely hesitated to offer explanations, as interpretations, of the factors they discover, but are often criticized for hindsight. The issues, however, have never been properly examined from a logical or philosophy-of-science standpoint. Albino (1953) has had something to say in this direction, but there is much in the work of Peirce (1950), Carnap (1936) and others which has application to factor methods, and which has long been overlooked.

For the present, following Carnap, it is perhaps reasonable to proceed as follows. Any interpretation of a factor is a definition. But one should distinguish between explicit definitions and convergent reduction chain definitions. When Thurstone finds a primary factor for a *numbers* test, to call it a primary ability is merely to give the test an explicit definition. Nothing about the *numbers* test is explained in any hypothetical way. The situation can be very different for a reduction definition. To put salt in water (to use the familiar example) is a test condition (*Tc*); to see it dissolve is the test result (*Tr*). To say that the salt is *soluble*, however, is to attribute to the salt-in-water an attribute or characteristic (*Qs*) with the status of an *ad hoc* explanation. We say, likewise, that lead is *insoluble* in water, and this has the same *ad hoc* status. But solubility might be a fruitful term, and insolubility not; one may wonder whether colloidal, amorphous mixes, or other physical or chemical properties, are involved when salt dissolves in water, but there seems to be nothing to explain about the lead-in-water. Interest in science is an *ad hoc* explanation where the conditions provoke curiosity, and where there are immediate possibilities for hypotheses about the *Qs* term. Thus, if it is suspected that salt-in-water is an amorphous mix and not a colloid, the matter would have to be discussed theoretically, deductions would be drawn, and methods sought for putting these to empirical test.

I believe that it is true to say that few provoking reductions have been found in factor studies of abilities, whereas they are apt to run riot in studies of personality. Thus, if tests *A* and *B* are (respectively) tests of *numbers* and *colour dominance*, and define one and the same primary factor *f*, the conditions exist for curiosity. The test result is that *A* and *B* are testing the same thing; and explanation is required, then, which apparently has to transcend the explicit definitions of the two tests—neither numbers, nor colour dominance can explain the factor. Thus when Albino (1953) argues that the definition of tests by *simple structure* is tautologous, a matter of explicit definitions only, the trouble is not with simple structure as such, but with the fact that startling reductions do not, apparently, occur for tests of ability. The analysis is into fragments, and not into provocative reduction terms. The situation is logically far more promising for second-order factors, since each embraces a number of different primaries; but few factorists give these much concern, on the grounds, apparently, that the basic explanations lie with the primaries. In any case, second-order factor reductions may or may not be fruitful—the situation may be like that for lead-in-water rather than for salt-in-water. Matters are more provocative in

personality studies, where convergent reduction terms are prolific, and where, correspondingly, there should be all the greater inducement to pursue scientific work which will be more than an undertaking with sets of explicit definitions. Thus, in a *Q*-technique study, if *A* and *B* define a factor *f* and *A* is a person's concept of *himself*, and *B* is his concept of what *God* is like (the concepts being defined operationally as *Q*-sorts), the somewhat presumptuous result may compel our wonder: the person of course may be saintly; but he may be deluded as well; or an all-consuming conceit may be at issue. Any such interpretation is an *ad hoc* reduction (*Q*s), and one or other may be fruitful, and the way is open for exploring matters further, with hypotheses that lead to new test conditions. Thurstone has, of course, always agreed that any interpretation of a factor is merely a beginning towards a scientific explanation; but here we perhaps see that factors may be such that they are either merely explicit definitions, or that, even if they are reductions to *ad hoc* explanations they may be sterile and incapable of hypothetical treatment. What we require, therefore, are *ad hoc* reductions which can be given immediate theoretical treatment, and it is fair to suppose that only those which compel a certain curiosity are likely to be amenable to such elaboration.

Cattell, in fact, interprets several factors by reduction terms. His factor H (1946*a*, p. 328) is called *adventurous cyclothymic* versus *withdrawn schizothymic*. It embraces such traits as *kindly, idealistic, open, sentimental, lusty, trustful, expressive* and the like on the one side, and on the other *cynical, schizoid, spiteful, aloof, misanthropic, timid, paranoid, narrow*, etc. That these traits cluster in the factor space is a test result; and in so far as they have different meanings and are not merely synonymous in the dictionary sense, they require a reduction explanation. One has to ask, therefore, whether *cyclothymic* versus *schizothymic* is a provocative reduction term. It is clearly not, if it does not lend itself immediately to hypothetical treatment leading to testable propositions. Cattell offers no such treatment. His principle surface traits (Cattell, 1946*a*) are analyzed into primaries, much as brown can be analysed into the three primary colours. Complicated forms of behaviour are thus presumed to be composed of various combinations of twelve or so primaries. There, somehow, the matter rests; but this is not because one's curiosity has been assuaged—rather, it has perhaps not been stimulated.

IV. A THEORY OF VALUE-SYSTEM MODES OF REGARD

The material situation concerns *X*'s who observe others, the *Y*'s, in relation to trait names. I am to suppose that the *X*'s have a few basic modes of regard, of a value-system order, and that these explain complex traits.

The theory follows the lines of self-psychology, such as stems from Cooley (1909) or Mead (1934). The *X*'s are no longer regarded as 'objective' sounding boards, reflecting 'objectively' what they see of the *Y*'s. Rather, behaviour of a self-referent kind is at issue, such as one finds in the *me-you* dynamisms of Sullivan (1940)—the husband, it will be remembered, who thinks of himself as *amiable*, thinks of his wife, correspondingly, as *devoted* to him. Thus, in trait analysis one has to find what *me*'s, so to speak, go with what *you*'s in the dynamic situation linking the *X*'s and the *Y*'s. The *X*'s know the *Y*'s very well, as Cattell also supposes; I would perhaps like to add that they know their *kind* very well. I assume that the *X*'s assess the *Y*'s within the framework of a value system, for the main part. They regard the *Y*'s as *unfair, naughty, goody-goody* or the like, as though

moral standards are at issue. One might *look*, therefore, for factors with such explanations. The *Y*'s might also notice the very obvious fact that some *Y*'s run about more than others, and thus seem more *active*; only very obvious 'objective' facts of this kind are likely to be observed spontaneously by *X*'s.

The theory, then, proceeds as follows: First, traits are considered to be meanings, and not general propositions or actuarial assumptions about behaviour. The point has been made before by Kantor (1933) and myself (Stephenson, 1953). Secondly, it is proposed that the *X*'s assess the *Y*'s by way of a system of *modes* of regard, there being relatively few basic modes, of a simple-minded or very obvious kind. Thirdly, the system is such that complex traits may be supposed to develop out of these simple ones in a coadunating fashion, which, in its simplest form, may be compared to a mechanical relay system such as Tustin (1953) and others have described for analogous brain conditions.

With respect to modes of regard: these are reached concretely in *Q*-technique studies, much as Freidenberg & Roth (1954) indicate for the conceptions of university students about their campus milieu, or as I have discussed for *Q*-sort descriptions of personality (Stephenson, 1953). Factors, for such *Q*-sort descriptions, represent modes of regarding others in relation to oneself. They are learnt ostensibly, in a social milieu, and may be compared to types or models—what we think the typical Frenchman is like, or the typical milkman, or the typical English schoolboy, are examples of the kind of formations here postulated. They are total, global affairs, reflecting the motivations and prejudices of the *X*'s. They are likely to involve moral evaluations in Cattell's study; the concern will be with the typical *naughty* student, or the 'dumb blonde' one, or the like. They are total in the sense that it takes not less than a *Q*-sort, for a large sample of traits, to describe them adequately. A verbal tag may be given to these modes, e.g. 'dumb blonde'. They are clearly not ultimate in any sense; nor need they be fixed things—they may change, as Hebb (1949) suggests for trait names, and yet retain their verbal tag. Here it is supposed, however, that some modes are rooted in earlier development than others. The modes correspond roughly to Mead's 'generalized others'.

The third prop of the theory is that complex traits are compounded from or linked with a few such basic modes of regard. The few may be regarded as keys in a mechanical relay system, leading to networks which are reached by pressing this or that combination of the keys; in this manner one proceeds, neurologically, from the schema concerned with basic modes to those for more complex traits. The model has been employed often before, for example, by Tustin (1953) and McCulloch (1944).

The material system, then, is conceived as meanings that *X*'s have of 'generalized others'; some of Cattell's traits refer to these as verbal tags. Along *Q*-technique lines these 'others' can be demonstrated facts; for Cattell's data, however, they have to be inferred. Combinations of these occur in a relay system fashion, which at least does not contradict what would be called, nowadays, sensible 'brain talk' (Young, 1951).

V. THE FORMAL MODEL

The formal part of the theory, from which the deductive and testing possibilities stem, and which covers the material system isomorphically, is a system of factors, as follows. Let there be a small number of orthogonal factors, say *A*, *B*, *C* and *D*. These can be

combined none, one, or more factors at a time. For positive and negative loadings (such as occur for bipolar factors), the arrangements are as follows:

Null-conditions: none of the factors.

Single conditions: A, B, C, D and correspondingly, $-A, -B, -C, -D$.

Two factors: AB, AC, AD, BC, BD, CD , each with four possible arrangements of the signs, e.g. $AB, (-A)B, A(-B), (-A)(-B)$.

Three factors: ABC, ABD, ACD, BCD , each with eight possible arrangements, e.g. $(-A)BC, A(-B)C$, etc.

Four factors: $ABCD$, with sixteen possible arrangements of the signs, e.g. $(-A)BCD, (-A)(-B)CD, \dots$, etc.

The rules for making deductions are as follows. For bipolar traits, given any combination of the factors, the poles consist of the same factors with signs reversed. Thus, $A(-B)(-C)$ is the opposite pole of a trait whose factor composition is $(-A)(B)(C)$. Another deductive possibility is that certain combinations of factors are mutually incompatible, and therefore do not occur empirically. These deductions are carried out within a system which may or may not correspond to dictionary definitions of traits, but all lead to testable propositions. As just stated, $A(-B)(-C)$ is the opposite pole of a trait whose factor composition is $(-A)(B)(C)$. If *hypochondriacal* has the latter factors, a trait with the diametrically opposite meaning will have the former. Dictionaries have been unable, in the past, to say what is the opposite of *hypochondriacal*, except to say that it is *not-hypochondriacal*—which of course could cover almost anything in the world. We may be able to deduce, however, given the primaries A, B, C , that *punctilious* should have a factor composition diametrically opposite of that for *hypochondriacal*. It is then a simple matter to put the deduction to empirical test. If the trait *punctilious* does not appear in Cattell's list of traits, one would include it in a new study of traits, to see whether the predicted factor composition occurs for *punctilious*. Again, if it is deduced that no trait can occur with the factor composition CD , the fact that this combination has not occurred previously in factor studies, or that it does not occur in studies designed to test it, is evidence that the deduction is on the right lines. The theory as a whole, then, will be the more credible the greater the number of such deductions one has put to empirical proof.

The idea of a network in the material system, clearly, is here modelled by factors, where the factors correspond to basic modes of regard, and compounds of these to complex traits.

VI. RE-ROTATION OF CATTELL'S CENTROID FACTORS

It will be recalled that Cattell or his assistants spent the equivalent of one man-year of calculations for his solution to the multiple-factor problem for the twelve centroid factors extracted from the data for the thirty-six traits (Cattell, 1947), and much longer than this for other comparable data. Banks (1948) refactored the latter table, spending a month's work upon the solution. My own solution, with a theory to guide the rotations, takes less than two hours for either of Cattell's tables. It proceeds as follows, using Table 1 of Cattell's 1947 paper for the present explication.

A solution is required which will satisfy these conditions: orthogonality will be retained, in keeping with the requirements of the formal factor model; *simplest structure* (Stephen-

son, 1953) will be looked for, and not *simple* in Thurstone's sense (Thurstone, 1947a). A third condition for an acceptable rotation concerns the rules for making the dependent factor analysis; here one argues, from the self-theoretical framework of the *X*'s, that such traits as *jealous* and *suspicious* are likely to hang together about a primitive mode of regard. One would expect, on the same commonsense grounds, that *easily upset* and *anxious* (or *worrying*) would also so hang together, and likewise *alertness* and *energetic*. Each pair, indeed, could almost suggest a little dynamic system for itself, in which the terms relate as causes and effects. For the moment, however, it is expected that *jealous* and *suspicious* will have reference, for naïve *X*'s, to students who are of a somewhat depraved kind, the 'naughty' people, so to speak, of the campus. Similar considerations apply to *anxious* and *easily upset*; these must surely be key verbal tags for a stereotypical regard of certain 'others' who are readily recognized by *X*'s. The same applies for the *alert*, *quick*, *energetic*, *lively* personality; what *X*'s are unlikely to have a stereotype of such a high-tempered clavicord? Now I do not know precisely which of such modes will occur empirically for Cattell's data; but the facts can be looked at with expectancies of the above kind in mind (a scientific procedure about which I have written elsewhere as an example of the logic of abduction, a method of inference originally outlined by the American philosopher C. S. Peirce (1950) and discounted ever since), and the rotations of Cattell's centroid factors made in accordance with such expectancies. That is, the necessary factors are reached by *observing* that certain variates hang together as a factor, as *expected*. Another deduction involves variate no. 36 (the intelligence of the *Y*'s as objectively measured): one assumes that it must tend to be orthogonal to most personality traits, and, therefore, no solution to the rotations will be acceptable which does not have no. 36 distinctly on a factor which is orthogonal to all other factors, and which does not 'make sense' in relation to other traits.

With these conditions in mind, then, one proceeds with the rotations. It is scarcely necessary to outline the steps taken to reach a solution, except to say the following. As Banks (1948) notes, the bipolarities for Cattell's traits do not all run in the same direction, resulting in many negative correlations which confuse matters somewhat unnecessarily. Reversals were therefore made, it being sufficient to do so for Cattell's traits nos. 1, 2, 5, 7, 9, 12, 13, 15, 16, 18 and 27. Using the graphical method (Zimmermann, 1946) for the centroids two at a time, the solution is very simply reached by using trait no. 11 (*suspicious*) to define factor *A*, and therefore also *B* (since the factors are orthogonal), and no. 26 (*anxious*, *worrying*) to define *C* and *D*. That is, Cattell's first four centroid factors are rotated to *A*, *B*, *C* and *D* respectively, using these few traits as guide-posts for the dependency reasons to which allusion has already been made. It is then observed that Cattell's sixth centroid relates to *D*, and by way of traits nos. 8 and 26 as the defining traits, a factor *E* is assumed. Cattell's seventh centroid, likewise, relates to *C*, and allows us to place trait no. 36 on a factor *F*, *C* being swung round to be orthogonal to *F*. The remaining centroids (nos. 5, 8, 9, 10, 11 and 12) offer no scope for dependency rotating, and are therefore discarded. It will be noted that Banks also found that she could dispense with many of Cattell's centroid factors, and that Thomson (1947) had earlier reached the same conclusion. We end, then, with six orthogonal factors *A-F*, five of which are well enough defined. The solution is given in Table 1. If loadings of 0.30 or more are considered to be significant, the typification at the right of Table 1 covers the data. Traits 11, 22 and 24 define *A*; traits 9, 16, 25, 32 and 34 define *B*; traits 17, 20, 21 and 35, define *C*; traits 8

and 26 factor *D*. The intelligence test focuses factor *F* for us, and *E* appears only in combination with other factors. The situation is one of *simplest* structure in our sense (Stephenson, 1953).

Table 1. *Cattell factors rotated to simplest structure*

(Involving the first seven factors of Cattell's unrotated, centroid factor matrix (Cattell, 1947, p. 207).)

Variables	<i>A</i>	<i>B</i>	<i>C</i>	<i>D</i>	<i>E</i>	<i>F</i>	Typification
1	51	-25	24	-38	-22	-05	<i>A</i> (- <i>D</i>)
2	42	10	66	-01	-10	-24	<i>AC</i>
3	54	65	18	16	06	-20	<i>AB</i>
4	41	60	-35	-25	-03	01	<i>AB</i> (- <i>C</i>)
5	-31	59	24	00	14	-09	(- <i>A</i>) <i>B</i>
6	52	22	33	15	03	-32	<i>AC</i>
7	-32	53	-09	41	15	09	(- <i>A</i>) <i>BD</i>
8	21	-13	24	68	01	-14	<i>D</i>
9	-29	70	-14	-12	-02	08	<i>B</i>
10	21	-17	55	16	-36	-44	<i>C</i> (- <i>E</i>)(- <i>F</i>)
11	74	-04	03	11	06	01	<i>A</i>
12	65	-07	03	-35	-21	-04	<i>A</i> (- <i>D</i>)
13	33	43	43	03	14	-04	<i>ABC</i>
14	38	-02	29	23	35	-01	<i>AE</i>
15	58	37	31	-26	-20	01	<i>ABC</i>
16	10	80	20	00	-11	-10	<i>B</i>
17	17	-11	78	05	-13	-07	<i>C</i>
18	-11	74	40	-16	-12	03	<i>BC</i>
19	44	07	-34	-38	-16	19	<i>A</i> (- <i>C</i>)(- <i>D</i>)
20	04	19	81	-08	06	16	<i>C</i>
21	11	-17	53	-23	24	-16	<i>C</i>
22	76	17	06	16	14	17	<i>A</i>
23	64	-32	-27	-01	09	13	<i>A</i> (- <i>B</i>)
24	74	26	16	-10	-00	-13	<i>A</i>
25	25	50	-05	-08	20	-03	<i>B</i>
26	26	-04	21	47	-01	00	<i>B</i>
27	36	31	51	-26	-41	07	<i>ABC</i> (- <i>E</i>)
28	05	77	-19	-16	31	-03	<i>BE</i>
29	13	25	-04	31	45	08	<i>DE</i>
30	51	-01	-07	36	37	-09	<i>ADE</i>
31	00	34	-27	24	18	16	<i>B</i> (- <i>C</i>) <i>D</i>
32	00	46	20	07	06	-06	<i>B</i>
33	18	62	-08	04	13	03	<i>B</i>
34	03	74	26	07	05	-20	<i>B</i>
35	15	-20	68	26	13	-09	<i>C</i>
36	-09	04	01	16	12	57	<i>F</i>

(Decimal points omitted.)

VII. THE REDUCTIONS

The factors are now interpreted. Factor *A* involves the traits *suspicious*, *jealous*, *demanding* (with the bipolar counterparts *free from suspicion and jealousy*, and *emotionally mature*, or *thinks of others*). I propose that the generalized factor hypothetically at issue in this case is concerned with a 'naughty' versus a 'gracious' young woman; a 'nasty bit of work' type of woman, perhaps, as contrasted with one of poise and good breeding. Factor *B* clearly requires 'vital' versus 'quiescent' as the reduction term. On the positive side of the factor are the traits *energetic*, *talkative*, *unconventional*, *eccentric*, *frank*, *expressive*, *gregarious*, *sociable*, with *marked interest in opposite sex*. On the negative side, contrasting with this, are the traits *languid*, *slow*, *silent*, *introspective*, *secretive*, *reserved*, *self-contained* (*goes own way*), and of course with *slight interest in the opposite sex*. The

'socialite' is perhaps the type on the one side, and the 'misanthrope' or the like type on the other side. Factor *C* is reduced to an 'immature, somewhat crude' person versus an 'independent, somewhat cultured one'; the traits are *fickle, indolent, awkward, immature, dependent* versus *persevering, tidy, polished, independent-minded, charming*. Factor *D* suggests the 'easily upsettable' person versus the 'tough' one. The traits are *easily upset, anxious, worrying*, versus *unshakable poise, calm, tough*; the 'neurotic' type versus the 'tough guy' comes at once to mind as the generalized others. Finally, for *E* there are no simply defining traits, but nos. 29, 10 and 27 are involved, and from these I would offer the following reduction term: 'impractical, sensitive' versus 'logical'—the 'day-dreamer' type, as compared with the 'thinker'. Factor *F* is the objective measure of intelligence.

It is to be remembered that the traits were defined, in the original study by examples and a brief paragraph of description. One must refer to Cattell's paper for a clear notion of what the various traits may be supposed to have meant to the *X*'s. By the same token it is impossible to find any precise verbal tag for the factors, that is, any sufficiently comprehensive word to cover what the reductions involve. All that is required is that the reduction terms point to concrete modes of regard of the *X*'s, that is, to generalized others. The term 'naughty', that is, indicates that the *X*'s carry about with them a concept of certain others in their milieu who are not nice, a little depraved, and the like. The concern is to see these as naïve but concrete matters, such as young students might well have in mind about their peers.

VIII. COMPOUND TRAITS

Whereas Cattell, with his twelve primaries, covered the thirty-six traits more or less precisely by *simple* structure, there being no trait with two or more factors, the structure for our six centroids, rotated, embraces only about one-half in the *simple* condition, leaving the other half to be involved with two or more of the primaries *A–F*. Compound traits are defined as those having two or more of the factors *A–F*. It is proposed that compound traits receive an explanation, in a hypothetical sense, in terms of the basic factors.

Consider a few examples. We see from Table 1 that *hypochondriacal* is compound (*AC*): it is made up of 'naughty' plus 'immature'. *Obstructive* (no. 1) has factors *A* and *–D*, and thus involves 'naughty' plus 'tough'. The trait *somewhat unscrupulous* (no. 27) has factors *ABC(–E)*, and has the meaning, therefore, in terms of the primaries, of 'naughty' plus 'vital' plus 'crude' plus 'logical'. If the reader will muse about these terms for a moment I think that it will occur to him that *somewhat unscrupulous* could well have the character of being somewhat depraved ('naughty') but full-blooded ('vital'), and yet also unpolished and lacking in charm ('crude') as well as being scheming and calculating in the way of a 'practical, logical' mind. Again, trait no. 19, for *stern, hard*, has the factor composition *A(–C)(–D)*, meaning 'naughty' plus 'independent' plus 'tough'. The opposite pole, for *kindly, softhearted*, is reduced to 'gracious' plus 'immature' plus 'upsettable'.

The reader must determine for himself how far the composites 'make sense' for the various compound traits of the Cattell data. Meanings of words are notoriously slippery, but there is surely more than fortuitousness in the reductions here under consideration. It is amusing, at least, to see how closely the composites 'hit' the meanings of complex traits, and this, I venture to say, is without exception in the data. The *boor*, for example, is amusingly indicated as *C(–E)(–F)*, that is, 'crude' plus 'logical' plus 'somewhat unintelligent'—showing that *intelligence* as objectively measured, can enter the present

scheme of things, contrary to what Banks supposed. But perhaps the most interesting trait in the literature on personality is no. 13, for *emotional* versus *calm*, *phlegmatic*, that is, concerning the way some people become angry, fearful, jolly, sexual, sorrowful, or express disgust and the like at the slightest provocation; who are frequently emotional and passionately excited; who show marked signs of emotion, even if it is under some control. This trait has factors *ABC* here, yet all factorists up to now have concluded that it is of a fundamental and primary nature. For Burt it is the general factor of personality and Spearman held a similar belief, except that he believed that *lack of control* was more essentially at issue (his *w*-factor); but Thurstone, Cattell and Eysenck, amongst others, accept the primary nature of such a factor. Here it is grasped as 'naughty' *plus* 'vital' *plus* 'crude' or 'immature', and these seem to characterize the matter very well. The psychologist has assumed previously that conditions of emotion, of sex, jollity, sorrow and the like *should* be basic to personality, and therefore has acquiesced in the notion of a general, or also of a primary, factor of *emotionality*; he did so without proof. It no doubt seems a reasonable interpretation to make. Yet I fail to see the need for it—on the contrary, the key word *emotional* stands, for the *X*'s, for their conception of a person who is somewhat depraved, yet vital, and of course immature and crude; indeed, perhaps the concept *emotional* is derived from such basic conceptions. The *reasons* for any emotionality of the *Y*'s are quite another matter; for the *R*-factorists the reasons *are* the emotionality as such, as a primary fund of something inside the *Y*'s, with a physiological origin and roots. This, however, is perhaps about as far from an adequate reason as anything could be. Spearman, in postulating a factor of self and control (*w*-factor), was on better lines. The emotional quality of a person is a matter for concrete study in the concrete case; to regard behaviour as in some way explained by general emotionality, or by primary emotion, is not a genuine hypothesis; it does not deal with the facts as they can be observed, and it serves (as such theories are apt to do) to stop the inquiring scientist from discovering these facts for himself.

Be this as it may, I provide the complete list of the compound traits for Cattell's data, with their reduction to the basic factors. The data are in Table 2.

It should be remembered, perhaps, that the 'cut-off' of acceptable statistical significance for factor loadings, assumed to be 0.30 above, is somewhat arbitrary. The standard error of these loadings is difficult to assess (Thurstone, 1947*a*). I would draw attention, however, to the surprising way in which the above interpretations apply if the 'cut-off' is reduced. For this reason I have retained above a typification of trait no. 31 (*aesthetically fastidious*) with factors *B*, $-C$, *D* for a 'cut-off' at 0.20 loading; fastidiousness is grasped as 'vital' *plus* 'independent' *plus* 'upsettable', which together seem to make sense for the trait.

Certain of the combinations appear to be impossible empirically; thus, one can scarcely be 'vital' and 'quiescent' at the same time, and in the above system it is predicted that $A(-C)$, $B(-D)$, $AB(-D)$ will never occur in fact. They do not appear in Table 1.

IX. DISCUSSION

It is not supposed that only five or six basic modes of regard occur empirically, or that I have really hit upon adequate modes from Cattell's data, or that any mode is other than a condition of flux for the developing *X*'s. What is interesting, however, is the

Table 2. *Reduction table for compound traits*

Factor compositions	Variables	Compound traits		Corresponding reductions	
		(Positive)	(Negative)	(Positive)	(Negative)
B	3	Attention-getting (shows off, boastful)	v. Self-sufficient, under no wish to get attention	'naughty' + 'vital'	v. 'gracious' + 'quiescent'
A)B	5	Cheerful	v. Depressed, solemn (unhappy, dour)	'gracious' + 'vital'	v. 'naughty' + 'quiescent'
C	2	Changeable	v. Emotionally stable	'naughty'	v. 'gracious'
	6	Frivolous	v. Responsible	+ 'crude'	+ 'independent'
E	14	Hypochondriacal	v. Not so	'naughty' + 'impractical'	v. 'gracious' + 'logical-minded'
-D)	1	Obstructive	v. Ready to co-operate	'naughty'	v. 'gracious'
	12	Spiteful, grasping	v. Good-natured	+ 'tough'	+ 'sensitive'
E	28	Composed	v. Shy, bashful	'vital' + 'practical'	v. 'quiescent' + 'impractical' (sensitive)
C	18	Adventurous, bold	v. Cautious, retiring	'vital' + 'crude'	v. 'quiescent' + 'cultured'
-B)	23	Rigid, conservative	v. Adaptable, open to compromises	'naughty' + 'quiescent'	v. 'gracious' + 'vital'
E	29	Sensitively imaginative	v. Practical, logical	'upsettable' + 'quiescent'	v. 'tough' + 'logical'
BC	13	Emotional	v. Phlegmatic	'naughty'	v. 'gracious'
	15	Self-willed, egotistical	v. Mild, self-effacing	+ 'vital' + 'crude'	+ 'quiescent' + 'cultured'
B(-C)	4	Assertive, self-assured	v. Submissive, humble, embarrassable	'naughty' + 'vital' + 'cultured'	v. 'gracious' + 'quiescent' + 'crude'
A)BD	7	Attentive to people	v. Cool, aloof, coldly indifferent	'gracious' + 'vital' + 'sensitive'	v. 'naughty' + 'quiescent' + 'tough'
DE	30	Neurotic fatigue	v. Not so	'naughty' + 'upsettable' + 'sensitive'	v. 'gracious' + 'tough' + 'logical'
(-C)(-D)	19	Stern, objective	v. Kindly, soft-hearted	'naughty' + 'cultured' + 'tough'	v. 'gracious' + 'crude' + 'sensitive'
(-A)CD	31	Aesthetically fastidious	v. Lacking in artistic feeling	'vital' + 'cultured' + 'sensitive'	v. 'quiescent' + 'crude' + 'tough'
BC(-E)	27	Somewhat unscrupulous	v. Conscientious	'naughty' + 'vital' + 'crude' + 'impractical'	v. 'gracious' + 'quiescent' + 'cultured' + 'practical' (logical)

theoretical possibility that along such lines one could account for all the personality affections in Roget's *Thesaurus*. Ten basic factors, combining not more than four at a time, could account for over sixteen hundred traits of affection and their synonyms, and the number becomes astronomical for larger compoundings and a few more basic modes. Again, a basis exists for determining empirically what might be the appropriate synonyms for unusual traits, such as *frippery*, *serene*, *indecisive* and the like.

The differences between the above model and the group-factor solution adopted by Banks can help us to say in what sense the one is a scientific system and the other not. Banks accounts for Cattell's data by a general factor (e) and three bipolars, x , y , z . There are thus seven defining points in the factor model, e , x , $-x$, y , $-y$, z , $-z$. She draws attention to other traits which lie at the eight interstices between the three orthogonal bipolars, namely, at xyz , $(-x)yz$, $x(-y)z$, $xy(-z)$, $(-x)(-y)z$, $(-x)y(-z)$, $x(-y)(-z)$, $(-x)(-y)(-z)$, to which she gives the names paranoid, adolescent, hysteria, compulsive, anxiety, dominating, impulsive, cordial and facile (Banks, 1948, pp. 216-17). One wonders why traits are missing compounded of two of the factors, or of four, or why the *null* situation is omitted. Personality is merely diagrammatically represented as a 14-point star, held at the end of a general factor stick e . Surely there can be no such material thing, either in personality as such of anyone, or in 'brain talk'. No isomorphism can exist between the model and anything material.

Our own model, on the other hand, attempts to deal with material things, to which it has an obvious isomorphism. Everything concerns the internal frame of the X 's, who may be supposed to have generalized others in mind. The traits assessed by the X 's, therefore, are regarded as referrable, hypothetically, to modes of regarding others. All else then follows. This is not to say that one learns nothing about the Y 's in this way; but if one wants to learn about the emotions and temperamental qualities of the Y 's there are appropriate ways to do this, such that the models used are at least to some degree isomorphic with material systems at issue, and this is not the case for Cattell's primaries, nor for Bank's factors, nor for Eysenck's hierarchical systems (Eysenck, 1953).

The demonstration I have provided should be regarded, perhaps, as a plausible account of the way meanings, in this case concerning personality, may develop and coadunate. Given the key modes A and B , pressing the two together reaches into AB —so the complex traits have meaning given to them, almost neurologically it would seem. Or, traits are so organized as to meaning. It only remains to indicate in what the deductive framework consists. The concern is entirely with *meanings*. In our factor scheme, I infer that the trait *serene* should be $(-A)(-B)(-C)$, that is, 'gracious' *plus* 'quiescent' *plus* 'independent'. *Cowardly* should be ACD , that is, 'naughty' *plus* 'immature' *plus* 'upsettable'. The opposite to *cowardly* is $(-A)(-C)(-D)$, that is, 'gracious' *plus* 'independent' *plus* 'tough', which could well be *brave* in the sense of 'carrying oneself defiantly under suspicion or blame'. It is not *brave* in the sense of *showy*, or a *desperado*, for which the factors should be BC , and $ABC(-D)$ respectively. All such inferences are carried out by myself, a sort of integrator; each inference, however, can be empirically tested, merely by including these various traits in future studies of the Cattell kind. The predictions can only be as good as the interpreter is sound as an integrator. The formal deductive framework provided by the factor model, even so, is a decided help in the right direction, without which no predictions would be possible.

X. CONCLUSION

The self-same data have been differently interpreted by Cattell, Burt, Banks and Eysenck. All place the factors in the *Y*'s, the persons who are observed and rated by the *X*'s. I place the factors, instead, in the *X*'s. Those who look to factor analysis for scientific guidance may be pardoned, therefore, perhaps, for supposing that statistics can prove anything. However, the troubles are not statistical but logical and methodological. There has to be care for the material system at issue, for at least an approximation to isomorphism when this is covered by a model or metasystem, for what is operational and what not, for fruitful *ad hoc* explanations rather than merely explicit definitions, and for a genuine theoretical framework rather than for analysis into unrelated fragments of factors. In the above example the material system concerns modes of regard, generalized others, meanings, and the possibility of coadunation 'in the brain'. Factor analysis is a formal model. The theory, that simple modes of regard form first, and that from these complex traits are linked in a relay-key fashion, is highly tentative, fluid, an open invitation for further work, rather than a firm conclusion about anything. However, all one's interpretations can be empirically tested, which is a sound step indeed in any scientific work.

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SOME CHARACTERISTICS OF GOOD JUDGES OF OTHERS*

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A group of forty subjects, undergoing a 'living-in' assessment programme, were tested on their ability to make accurate analytic judgements about their peers. This ability was found to correlate positively with certain background variables such as urban origin, previous working experience, none or few siblings, being a student in the natural sciences (versus social science), and certain cognitive abilities. The good judges, as opposed to the poor ones, tended to be stable, serious and socially passive persons, task oriented and socially detached. No relationship was found between ability to judge others and artistic ability, ability to play roles, sociability, femininity or ability to judge self. The findings are explained in terms of a special ability, the availability of appropriate judgemental frames of reference, and the presence of motivation towards making objective social judgements. The good judges are typically extraceptive and the poor judges intraceptive.

I. INTRODUCTION

There is already an extensive literature pertaining to the ability to judge others, but much of it contains contradictory findings. This literature has been summarized by the writer (Taft, 1955) incorporating the findings presented below, and it will not be repeated in detail in the present report. The experiment described in this paper did not aim at testing hypotheses deduced from general psychological theory; its modest aim was to re-examine some of the long-cherished beliefs about the ability to judge others, to explore new areas of its correlates, and at the same time perhaps to reconcile some of the contradictions between previous studies. It is hoped, of course, that the empirical findings will indirectly advance the frontiers of knowledge about the operation of personality in general, and some suggestions along these lines are made in the discussion at the conclusion.

The experiment to be described resembles most nearly the study by Adams (1927), although techniques have been adopted from later experiments of a similar nature. The uniqueness of the present study lies in the fact that the judges were subject to an intensive personality investigation, and that, consequently, many more measures than usual were available for correlation with their ability to judge. *How imp. when?*

One limitation in the present study that should be borne in mind when evaluating the findings is the specialized type of judge used. It may well be that some of the characteristics found to discriminate between good and poor judges amongst advanced post-graduate students would not so discriminate amongst another sample of the population. It will be clear, however, that many of the findings of this study may be extended in their application far beyond the limited type of subject used here.

II. SUBJECTS AND ASSESSMENT CONDITIONS

There were forty subjects consisting of four equal groups. Each group of ten were the subjects of a large-scale personality assessment investigation, and for that purpose they

* The writer conducted this study while he was a research assistant at the Institute of Personality Assessment and Research, University of California, Berkeley. He extends thanks to his colleagues at the Institute, and especially to Dr Donald W. MacKinnon, for making data available and for their advice and criticism. The study on which this paper is based has been reported in more detail in Taft (1950).

were in residence together for a week-end. The subjects were all male graduate students either on the verge of completing the senior degree in their course or recent Ph.D. graduates. They included students in the physical and biological sciences, social sciences, law, business administration and music. The age range was 23-35, with a mean of 28 years.

Few of the subjects were acquainted with one another prior to the assessment programme—only 15% of all possible pairs had known each other before—but during the course of the week-end they were afforded considerable opportunity to become acquainted. The tests of ability to judge others were administered at the end of the assessment programme after the subjects had been living together for almost 2 days.

III. THE TESTS OF ABILITY TO JUDGE OTHERS

(i) *Estimation of M.M.P.I. responses*

The subjects were presented with thirty M.M.P.I. items and were asked to predict 'how many of the ten assesseees today, including yourself, answered each question the same as yourself'. All questions required the subject to answer 'true' or 'false'.

This technique has been adapted from that of Travers (1941), who did not use the method specifically as a test of ability to judge other people.

Scoring. A *discrepancy* score was computed by totalling the errors made in the estimates for each item without regard to sign; thus the lower the discrepancy, the greater the accuracy.

Reliability. It was not possible to apply the test-retest check on the present subjects, but this was done on thirty-three students of social psychology tested twice at an interval of five days. The test-retest correlation for the discrepancy scores of these subjects was 0.82.

(ii) *Rating test*

The most widely used single method of testing ability to judge others has been the rating method, since this method possesses the advantage of forcing the judge to express his judgements in terms that enable his accuracy to be computed quantitatively.

Instructions. The subjects were required to rate their nine-fellow-assesseees and themselves on the six traits described below. A five-point scale was to be used according to defined standards; for example, a score of four means 'has above average amount (in the top 30% of male graduate students)'. The traits were defined as follows: -

Persuasiveness: Ability to persuade others to agree with and to carry out his suggestions or points of view.

Social assertiveness: Takes initiative in making suggestions and giving orders; forceful and does not readily submit to another person (but need not necessarily be persuasive).

Sociability: Seek company and interacts readily.

Carefulness: Not impulsive; weighs the issues thoroughly before making decisions and taking actions and is slow to take chances.

Drive: Willing to undertake ambitious tasks and at the same time would show concentration and drive in executing them.

Conformity: Tries to conform closely to the conventions and expectations of his group with respect to appearance, social behaviour and attitudes.

Scoring. Accuracy was measured by the total discrepancy for all six traits between the judge's estimates and the subjects 'true' ratings. The problem, of course, is to establish the 'true' rating. For this purpose, discrepancy scores were computed using two separate criteria.

A. *Staff rating criteria.* After the conclusion of each assessment, the ten staff members who had observed the assessee rated them on the rating form. These ratings were then pooled and the means computed. This method of deriving pooled judgements has the disadvantage that it attenuates the range of the ratings, and extreme ratings are thus very rarely obtained. To compensate for this, the mean ratings for each trait were forcibly distributed for each group to correspond with the distribution of all of the individual staff ratings for the group on the particular trait concerned. These distributed ratings were then used as the 'staff rating criteria'.

B. *Peer rating criteria.* The median ratings on each assessee that were made by the other nine assesseees were taken as the 'peer rating criteria'. The mean gives too much weight to extreme ratings possibly made by very poor judges, while the mode has the disadvantage that some of the distributions were bi-modal.

C. *Adjusted ratings—staff rating criteria.* The discrepancies on which this third index were based were computed after the judge's ratings on each trait had been adjusted to compensate for any systematic bias. This adjustment was made by adding to or subtracting the appropriate integer from all ratings made by a judge on a trait where his average under or over estimation per ratee was greater than 0.5. For example, if judge A's ratings of his nine subjects on Persuasiveness showed a total overestimation of 16, all of his ratings on this trait would be decreased by two and the discrepancies then re-computed. Wherever an adjustment was not possible owing to the ratings already being extreme (1 or 5), no adjustment was made.

It may be argued that since persons differ in the ease with which they may be judged, some of the judges had an easier task than others. For example, a person who was himself particularly difficult to judge would have an advantage over his peers in judging the others, since his peers' subjects include him. In order to compensate for this, the discrepancy scores obtained by each judge on each of the above three methods of scoring were adjusted for the 'judgability' of the subjects that he was judging. To make this adjustment, the discrepancy scores of each judge were divided by an index of the disagreement between the judges in judging each subject. Where a subject was difficult to judge the denominator in the formula for adjusted scores would be large, and thus the errors of a judge in rating him would not be weighted as heavily as his errors in rating a subject who was easy to judge.

The rating test provided three scores for each judge, each score representing his degree of accuracy in relation to the judgability of the subjects. These scores were: A, discrepancy—staff rating criteria; B, Discrepancy—peer rating criteria; C, Discrepancy—adjusted ratings.

Reliability. No retest was practicable within the assessment situation, and a split-half test would have rendered numbers that were too small. We were, however, able to rule out the possibility that the scores obtained could have occurred if the ratings were distributed over the five points of the scale according to a purely chance distribution. This possibility could fit our results less than once in a thousand cases.

(iii) *Mosaic matching test*

During the course of the assessment week-end, the assesseees were required to produce a mosaic design. These designs were put on display and the assesseees were asked to match them with the 'artists'. They were allowed three matchings for each design and their score was the number of correct matchings.

(iv) *Overall index of ability to judge others (J.O.)*

The intercorrelations between the five measures described above are set out in Table 1. It should be noted, of course, that the correlations between the scores derived from the rating test are spuriously high since they are all derived from the same raw data.

Table 1. *Intercorrelations of measures of ability to judge others*

	(N = 40)					Mean correlation
	1	2	3	4	5	
1. Estimation of M.M.P.I. responses	—	21	36	11	00	17
2. Rating test—Index A	21	—	72	81	07	45
3. Rating test—Index B	36	72	—	52	19	45
4. Rating test—Index C	11	81	52	—	11	39
5. Mosaic matching test	00	07	19	11	—	09

The mosaic matching test was omitted from the overall index, since the type of judgement required appeared to involve the use of non-analytic cues, whereas the other indices were more likely to depend on analytic cues. The estimation of M.M.P.I. responses test was included, since it appeared to test one facet of the ability to make analytic judgements about one's fellows, and it has a mean correlation with the other indices of 0.23 (omitting the matching test— P is 0.10).

The estimation of M.M.P.I. responses test was given a weighting of two, and the three rating test indices each a weighting of one. The total provided the J.O. measure (ability to judge others). Prior to totalling the above four scores, they were all converted to a normalized five-point scale so that their dispersions would be equivalent. This procedure may appear to coarsen the value of the J.O. index as a measure of ability to judge, but the four scores of which the index is composed are probably too unreliable to warrant a finer measure.

In the remainder of this report the term 'good judges of others' will refer to the thirteen subjects who obtained a score of 17 or more on J.O. (possible 25), and the term 'poor judges of others' will refer to the twelve subjects with a score of 13 or less. For convenience, the J.O. scores were converted into T scores with a mean of 50 and a s.d. of 10.

IV. RESULTS

(i) *Sociological and life history factors*

The small number of subjects makes it impossible to take into account interactions between the variables. These may be important, e.g. the subjects with a rural background tend to have more siblings than the urban subjects.

A. *Age.* The age range was only 23–35, and within these limits no differences in accuracy of judging were detectable for different ages.

B. *Socio-economic status and vocational experience.* There was no relation between ability to judge and present socio-economic status. Those who had been officers in the armed services tended to be superior judges to the others (not significant), as did those with full-time working experience ($P\ 0.08$). These tendencies may be related to social sophistication.

C. *Family background and minority status.* Those judges with an urban background were superior ($P\ 0.03$) to rural and small town, again possibly a function of social sophistication. Within the urban group the lower the status of father's occupation, the better the ability to judge ($P\ 0.03$). This can be explained in terms of the biased sample of judges being used: those from a poorer economic background who reach graduate school are a highly selected group; also they may have made a special effort to learn how to appraise others as a means of raising their own status.

The results for judges from minority groups throw light on the latter argument. Although the eight foreign students and Negroes were poorer judges than the others, the nine Jewish judges were significantly superior ($P\ 0.02$). Possibly the factors connected with ambition in the case of persons of lower status are operating here too. Jews have had a long history of cultural adaptation, and they may have developed modes of parent-child interaction that facilitate this process; they are also aided by their status both 'in' and 'outside' the prevailing culture. On the other hand, other minority group members without this tradition are under a handicap in judging their peers, most of whom are members of subcultural groups different from their own.

D. *Sibling status and parental press.* The good judges had an average of 1.4 siblings, while the poor judges had 2.8. The scores on the J.O. index are given for various groups in terms of number of siblings in Table 2. We see a definite tendency for ability to judge others to decrease when the number of siblings becomes more than three. Probably children brought up in large families associate mainly with their siblings and have less opportunity to learn adult modes of judging others. In this connexion we may note that Halmos (1952), in his study of solitude, found that adults who had had more than two siblings were more sociable than those with fewer siblings.

Table 2. *Relationship between ability to judge others (J.O. index) and number of siblings, sibling status and academic specialization*

J.O. index	No. of siblings			Sibling status				Academic specialization	
	0	1-3	4-6	Only	Oldest	Middle	Young- est	Physical and biological sciences	Social sciences
Mean T score	55.5	51.5	39.3	55.5	52.0	49.8	44.6	52.8 (56.2)*	47.0 (47.2)
No. above median	3†	16†	0	3†	9†	5	2	14 (14)	4† (4)
No. below median	0	12	7	0	5	7	7	5 (2)	13 (12)

* Figures in parentheses exclude foreign students.

† One case fell at median in each of these groups.

‡ Two cases fell at median.

This explanation is supported by the further findings in sibling status. There is a steady decline in ability to judge from only children to youngest ($P\ 0.01$). Apparently the greater degree of self-reliance, combined with the increased contact with parents enjoyed by only and oldest children, develops in them a capacity for appraising others such as is not developed by children with older siblings.

The findings with respect to family relationships, as far as they go, enable us to develop this point further. When asked to characterize the dominant attitude of their parents towards themselves, the good and poor judges differed in the responses which they gave about their mothers (but not their fathers). The good judges tended to characterize their mothers as over-solicitous, anxious, nagging and ambitious (for the subject) much more often than did the poor judges. There were twenty-five such entries for the thirteen good judges against only ten for the twelve poor judges. The fact that they cognized their mothers in this way is important to an understanding of the pressures upon them to learn to understand others in a realistic fashion. It is possible that the poor judges would have characterized their mothers in the same fashion as the good judges if they had learnt to appraise their parents objectively; but the fact that they may not have learned to do so could have resulted from a comparatively greater dependence upon their siblings than upon their parents. It might well be that the foundation stone upon which the ability to judge others accurately is built in the acquiring, at an early age, of the ability to appraise one's parents objectively.

(ii) *Academic specialization*

In view of the challenging findings (Luft, 1950) that physical science students tended to be better judges of others than psychologists, it is interesting to compare our physical science judges with the others. There were no psychology students amongst the assessees, but the physical (and biological) science students were significantly superior in judging to the social science students ($P\ 0.01$). In this regard it is interesting to note that Roe found that prominent physicists tended to be elder children (Roe, 1951, p. 130), and also that they tended to be socially isolated.

In fact only three of the twelve poor judges, were physical scientists, and of these, two were foreign students and the other was the only physical science subject who had not yet passed his Ph.D. preliminary examinations. On the other hand, there were only three social scientists amongst the thirteen good judges.

These findings support Luft's findings on the high ability of physical scientists to judge others. There apparently are some factors that determine the choice of one's occupation that are also related to the ability to judge others. Here we have our first suggestion of a characteristic attitude which will be noted in good judges of others in several further references. This attitude is one of dealing with people as objects of study in an objective fashion analogous to the study of inorganic or organic matter.

On the other hand, the poorer judges of others may have entered social sciences because they are interested in people *per se*. As we shall also have further occasion to mention, the poor judges are characterized by a dependence upon people for their emotional satisfactions and are thereby moved to judge them somewhat autistically.

(iii) *Cognitive abilities*

The correlations between ability to judge others and various measures of cognitive ability are set out in Table 3. Although none of them are significant at more than the 0.05 level, they all point to a low positive correlation. An index was computed to combine the P.M.A. and the cognitive tests in terms only of each subject's best performance—one good performance is indicative of 'g'. The scores on this index correlated $+0.37$ with the J.O. index ($P\ 0.02$).

(iv) *Academic ability*

Further support is given to the above picture when we consider ratings of the assesseees' 'academic ability'. These were obtained by pooling ratings made of the assesseees by their own departmental faculty members on two separate qualities: Potential Success ('likelihood that he will, in future years, contribute significantly to his field') and Originality ('his freshness of vision and creativity of thought'). The correlations of these ratings are 0.18 (0.30, omitting foreign students) and 0.12 respectively.

Table 3. *Product-moment correlations between ability to judge others and certain cognitive, artistic and dramatic abilities*

($N=40$, r of 0.31 significant at 5% level.)

Test	Correlation with J.O.
<i>Certain cognitive abilities</i>	
Primary mental abilities (word knowledge + reasoning + no. reasoning)	0.13
Miller Analogies Test	0.26
Gottschadt Figures	0.29
Street Gestalt	0.11
Puzzles*	0.28
S's best performance	0.37
<i>Artistic and dramatic abilities</i>	
Barron-Welsh Figure Preference (3)	-0.19
Gerard Test of Good Taste (4)	0.31
Aesthetic quality of mosaic production†	0.21
Importance of aesthetic values†	-0.16
Understanding of role played in role- playing test†	-0.03
Empathy with role played†	-0.06

* These puzzles were a combination of verbal and practical problems requiring the use of 'insight' for their solution—designed by R. S. Crutchfield and P. F. Dempsey.

† Rating by assessment staff.

In summary then there is a consistent low, but not statistically significant, positive relationship between various cognitive abilities and the ability to judge others. At the same time it should be remembered that our group is particularly highly selected on intelligence.

(v) *Artistic and dramatic ability*

A number of artistic tests were given to the assesseees, and the correlations are included in Table 3. The Gerard Test of Good Taste, the only test to show a significant correlation, requires the subject to choose between simple abstract figures (Gerard, 1946). The criterion of good taste used by the author of this test was the judgements of 300 'experts' (probably commercial artists, although this point is not clear) modified in the light of those responses which served to distinguish college graduates and students from persons of lesser education.

The results reported in Table 3 suggest that the ability to judge others correlates positively with ability to follow simple traditional artistic rules (Gerard Test and Quality of Mosaic Productions as assessed by lay judges), but not with a more sophisticated and labile artistic attitude. It might well be that professional artists would perform poorly on tests of ability to judge others.

Similarly, no correlation is found between ability to judge others and measures of the assesseses' ability to understand and empathize with certain assigned roles in a role-playing test.

(vi) *Personality traits*

A. *Manifest personality.* The assesseses rated themselves on a check list of 279 descriptive adjectives (prepared by H. G. Gough), and at the conclusion of each assessment period the staff also rated them on the same check list. Using these latter ratings as the criteria the following differences were found.

Those adjectives that characterized the good judges significantly more often than the poor judges: 0.01 level (0.001 level in italics):

alert, calm, capable, *cautious*, *clear-thinking*, efficient, honest, intelligent, *logical*, organized, persevering, planful, practical, quiet, *realistic*, reliable, *reserved*, *serious*, *sincere*, *thorough*.

Supplementary list significant at 0.10 level:

aloof, awkward, conscientious, conservative, cool, dependable, determined, fair-minded, gentle, humourless, independent, industrious, moderate, painstaking, precise, rational, reasonably, unaffected.

Those adjectives that characterized the poor judges more often; 0.01 level:

affected, *argumentative*, changeable, demanding, excitable, hasty, headstrong, imaginative, irresponsible, obnoxious, opinionated, outgoing, pleasure-seeking, self-seeking, show-off, suggestible, *talkative*, *unstable*, undependable.

Supplementary list significant at the 0.10 level:

careless, civilized, dependent, dominant, egotistical, emotional, high-strung, humorous, immature, individualistic, simple, sophisticated, undependable, wary, witty.

Thus we get a picture of the good judge of others as a serious, organized and reasonable person who apparently relies upon the use of his intelligence and conservatism in meeting successfully the hazards of life. On the other hand, the poor judge is socially oriented, but not socially adjusted.

Support for this picture is offered by the subjects' own checks on the adjective list. The good judges prefer to use terms that indicate achievement orientation at the expense of social relations, e.g. industrious, logical, painstaking, reserved and retiring. The poor judges preferred adjectives almost all of which reveal social orientation, e.g. noisy, show-off, egotistical, emotional, affectionate, clever—but also careless.

B. *Masculinity-femininity.* Four measures of MF were obtained (M.M.P.I., M.N.E., (Gough, 1952), Franck Figure Preference Test (Franck, 1946) and Franck-Rosen Figure Completion Test (Franck & Rosen, 1949)), and none of the correlations with J.O. was significantly different from zero (range 0.00 to -0.12).

C. *Emotional stability.* We have already noted that in the Adjective Check List, the good judges were more often characterized as organized and stable. This is supported by overall ratings on Personal Soundness ('general effectiveness as a person, with emphasis on maturity and balance'). Ratings on this trait by the assessment staff correlated 0.29 with J.O. and 0.37 excluding foreign students (P 0.02). Ratings by the assesseses' departmental faculty members correlated 0.29 and 0.43 respectively (P 0.01).

A test of ability to find the vertical in a dark room* correlated 0.32 (P 0.05) with J.O.

* Based on the Asch-Witkin technique (Witkin & Asch, 1948) modified by R. S. Crutchfield.

If the suggestions made by Witkin (1949) are to be accepted, this would indicate that the good judges are the more self-sufficient and stable.

D. *Self-insight*. If we define self-insight as the ability to judge oneself accurately it is possible to compute a J.S. index corresponding to the J.O. index. This index included measures of the subject's ability to rate his own traits accurately, to predict his standing on the M.M.P.I. items in relation to the group's responses, and to predict his sociometric peer status. A correlation of only 0.11 was found between these two indices, suggesting that there is no relationship between the ability to judge others and self-insight. Actually the relationship is a complicated one and is non-linear; the poor judges of others tend to be either good or poor judges of themselves, while the good judges of others tend to neither extreme. Six of the twelve poor judges of others show what might be termed an inadequate degree of self-insight in comparison with only one of the thirteen good judges.

(vii) *Social relations*

A. *Attitude towards others*. We have already observed in connexion with the Adjective Check List that the poor judges of others appear to be more concerned with qualities relating to social relations, while the good judges are more achievement oriented. It has been suggested that these and other differences are indicative of a socially dependent attitude on the part of poor judges and a socially detached one on the part of good judges. Some further support is provided by the negative correlations between ability to judge and ratings by the assessment staff on exhibitionism (-0.26 , $P\ 0.10$) and sociability (-0.22 , $P\ 0.20$).

Much is revealed about social attitudes if we consider that the poor judges were much more likely to make errors in the direction of over-generosity than were the good judges. To measure this tendency, an index of generosity was computed according to the degree to which errors made in the rating test were errors of over-rating or of under-rating. (It was assumed that a high rating on all of the six traits—persuasiveness, social assertiveness, sociability, carefulness, drive, conformity—was a favourable one, although this is open to doubt in the case of conformity.) The generosity index correlated -0.48 ($P\ 0.01$) with J.O.

This suggests that there might be unwillingness on the part of the poor judges to risk removing their social supports by perceiving other people as they really are.

It is interesting to note that the difference in attitude towards their own social relations that we have detected in this and the previous section does not appear to carry over into a like or dislike of the company of others. The good judges tend to belong to as many social organizations as do the poor judges. Also there is no difference in the number of friends claimed by the two groups. Apparently the good judges of others enjoy contacts with other people and are by no means lacking in experience of such contacts. We are reminded of the distinction made by Asch between 'social interest' and 'social feeling' (1952, p. 335).

B. *Social skill*. Since the poor judges are more motivated to manipulate others to achieve their own purposes, particularly to receive recognition, it is of interest to note with what success they attain this. Judging by the Adjective Check List the poor judges do not seem to be any more successful than the good ones in attaining harmonious social relations. The good judges at least manage to avoid being characterized as 'obnoxious'! Ratings by the staff on social ascendancy, social relations and persuasiveness are not correlated significantly with J.O. (-0.08 , $+0.13$ and -0.11 respectively). Sociometric

choices by their fellow-assesseees on likeability as a friend correlates 0.03 with J.O. The only instance in which any significant correlation was found between J.O. and Social Skill was the rating on 'ingenuity' in the role-playing test where the correlation was -0.33. This correlation was probably a product of the poor judges' exhibitionism which led to their structuring their assigned roles in an original manner. In general, we can conclude that there is little relation between ability to judge others and social skill; certainly there is no evidence that high ability to judge other people is an aid in handling them.

V. CONCLUSIONS AND DISCUSSION

The correlations reported with ability to judge others may be explained in terms of one or more of three variables: cognitive ability, appropriate frame of reference and social attitudes. Cognitive ability barely enters into the picture in the highly selected group used here, but it appears to have some relevance; and this is only to be expected since an intellectual judgement is required. Frame of reference is suggested in the finding that rural and minority groups, and those without working experience, are poorer judges—they might easily have more 'ethical inhibitions' about judging their fellows as well as fewer opportunities to acquire the appropriate frames of reference. The same can be said of those subjects who were younger children. Also the poor judges tend to be less stable and organized, and they probably have less stable frames of reference.

Social attitudes, however, appear to be the most important variable; poor and good judges have been shown to vary in the degree to which they are socially dependent or detached, and this difference can explain the differences found in sibling status, academic specialization, and tendency to be a generous rater.

Apart from sheer experience and ability, the ability to judge others can be thought of in functional terms—how it fits in with the judges' motivations and attitudes towards judging his peers. The typology which we have deduced for the good and poor judges of personality can be expressed in the terms used by Murray (1938, pp. 222-3). Our good judges are his endo-cathective + extrareceptive type, not intrareceptive as he seems to suggest; both intrareceptive types appear to correspond with our poor judges.

An interesting comparison with our typology of good judges is provided by a group of subjects described by Pemberton (1952) as a 'high flexibility of closure' group. These subjects were typically physical science students; they scored well on the Gottschaldt Figures, were inclined to be socially detached and denied that they were 'intuitive'. Flexibility of closure seems to describe one of the characteristics required in the act of making a considered analytic judgement about an object or person, and the similarity between Pemberton's results and our own are striking with one exception—her subjects are described as not 'systematic'.

In conclusion, I should like to suggest a possible explanation for the particular pattern of characteristics found in good judges of others. These characteristics are all ones which befit a person to adjust to our present technological-scientific ethos—physical scientists, intelligent, organized, stable, hard-headed, administrative, socially detached and not intuitive types. The explanation is that judging another person in analytic terms, treating him as an object, is just another manifestation of this ethos. But whether this explanation is sound or not is a question which will probably be illuminated more by anthropological evidence than by psychological.

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TOWARDS AN INFORMATION-FLOW MODEL OF HUMAN BEHAVIOUR*

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This paper is concerned with the behaviour possible in an information-flow system intended explicitly as a hypothetical model for comparison with the human information-handling system. A statistically self-organizing system is described in which not only normal homeostatic behaviour but also such activities as the invention of fruitful hypotheses, the imagination of fictitious situations, and the like would find a natural place. Discussion is confined mainly to the manner of concept-formation and concept-handling in such a system. It has been suggested that the correlate of perception (as distinct from reception) is activity which organizes an outwardly directed internal matching response to signals from receptors. This organizing activity amounts logically to an internal representation of the feature in the incoming signals to which it is adaptive, i.e. the feature which is thus 'perceived'.

A hierarchic structure is postulated wherein much of the organizing activity is concerned with modifying the probabilities of other activity. Abstract concepts and hypotheses are represented by 'sub-routines' of such organizing activity. These can in principle be evolved as a result of experience in a manner analogous to—or at least fruitfully comparable with—the process of learning and discovery; and it is not necessary for the designer to predetermine, nor possible for him to foresee, all the conceptual categories in terms of which the information received by the system may be structured. Particular attention is directed to the conditions under which such a process of concept-formation could take place with sufficient rapidity.

Some of the symptoms of psychopathology find apparent correlates in possible modes of malfunction of such a system. But in this, as in other respects, it is not intended to press the resemblances; the intention is rather to stimulate critical comparisons, in order that the differences between the information-flow model and the real thing may continually lead to progressive refinement and enlarged understanding.

I. INTRODUCTORY

(i) *The wrong approach.* I suppose that the most superficial and unconvincing excuse for agreeing to discuss the behaviour of any artificial mechanism before a gathering of psychologists might run somewhat as follows: Experimental psychology has lately come to concern itself more and more with the human being as a 'black box', having certain characteristic ways of receiving and reacting to information (Hick, 1954). Perhaps not altogether accidentally, the merchant of automata has discovered that in his stock list there are several artificially contrived black boxes which in principle can be made to receive and react to information in the same characteristic ways. So, he might argue, his artificial black boxes are surely quite as proper subjects of the psychologist's attention as the human ones.

This argument seems to me, putting it mildly, to be mistaken. If the psychologist had *defined* his field as including 'all black boxes with a certain behaviour pattern', the excuse might hold. But if, as I believe, he defines it to be human (or animal) behaviour, then the merchant of automata has no automatic claim on his attention or even his interest. An automaton giving the most perfect imitation of observable human behaviour might prove to hold as much interest for the psychologist as a gramophone capable of reproducing human singing does for the laryngologist, and no more.

* Based on a paper read at the 14th International Congress of Psychology in Montreal, 11 June 1954.

(ii) *The conceptual role of the theory of automata.* So I must offer, tentatively, a rather different excuse. I believe that the theory of automata is potentially of use to the psychologist, not just because some automata could imitate the behaviour of his subjects, but because some aspects of the theory could, if properly disciplined, provide him with a useful tool of research.

What I think it offers is a kind of logical scaffolding, which may play a useful part in the processes of erecting, testing and (alas) demolishing psychological theories. We search nowadays for clues to the organization of human personality in two widely different fields, the provinces broadly speaking of psychology and physiology. These clues come expressed in characteristically different languages. Particularly in descriptions of pathological symptoms, it is often hard to imagine with any confidence the proper correlate in one language of data expressed in the other. We are convinced that the data *should* help us, but we have little idea what to make of them.

Now the language of information and control, in which the theory of automata is framed, is conceptually intermediate between those of psychology and physiology. It belongs in a sense to both fields, inasmuch as questions of information transfer arise in both.

It seems possible therefore that the theory of an automaton, specifically designed to be a research model of the human organism considered as an information flow system, might provide a common meeting-ground and a common language in which hypotheses might be framed and tested and progressively refined with the help of clues from both sides.

(iii) *The information-flow map.* For this purpose the physical engineering of the automaton is of little interest. It is sufficient if it can develop and grow on paper as a hypothetical information-flow map of the human organism. Just as the lines on a trade-route map of the world are drawn in the same way, whether goods are carried by train or lorry or camel caravan, so the pathways of our information-flow map may be postulated and tested and confirmed irrespective of the chemical, electrical or other details of the processes mediating the traffic in information.

On the other hand, the features of the information-flow map will normally suggest experimental questions in both the psychological and physiological fields, and should in turn find themselves tested and refined by the results.

This is what I meant by calling the theory of automata a logical scaffolding, potentially useful in building operations on psychological theories. Merely to ask what functions can be imitated, and how, would be pointless. The answer is that any pattern of behaviour that can be specified exactly or statistically can in principle be shown by an automaton, designed in one of any number of alternative ways (MacKay, 1951).

The real question is how far we can go towards designing a self-organizing system with the same internal information-flow map, as well as the same behaviour, as a human being.

We shall not find the answer in this paper. But I hope to show you that the answer is reasonable to seek, and not impossible to imagine, in dim outline, even now.

II. GOAL-GUIDED ACTIVITY

(i) *Minimal requirements.* Perhaps the most characteristic feature of intelligent behaviour is what we call goal-guided (goal-directed, adaptive, conative, purposive) activity. Let us begin by summarizing the essential features of any information-flow

system that is to show such activity. These features are by now familiar to most of us, and I have discussed them in detail elsewhere (MacKay, 1951, 1952). The minimal requirements are shown in outline in Fig. 1, where the activity of the system is represented simply by the movement of a point Y along a line F .

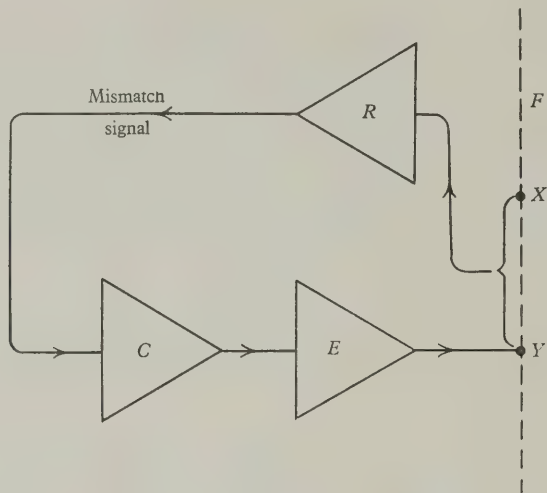


Fig. 1. The minimal requirements for goal-guided activity.

The active agent is represented by the 'effector system' E , which is governed by the control system C . We assume that in a typical situation E is capable of a certain range or variety of modes of activity (including inactivity), so that the function of C is to *select* from moment to moment what E shall do next, out of the range of possibilities open to it.

Suppose now that on the line F there is a point X , fixed or moving, towards which we want the activity of E to be 'goal-guided'. What do we mean by this? We mean (a) that the overall pattern of E 's activity should be calculated to reduce the interval between X and Y , or some time average of it, to a minimum. We mean (b) that if the interval were increased in any way (e.g. by movement of X away from Y , or by the action of some external agency forcing Y away from X) the effector E should actively resist the increase; but that it should actively assist any agency tending to reduce the interval.

The first of these requirements could in principle be met by a 'blind' automaton with a pre-computed control sequence, if the motions of the goal were known in advance. But in order to cope with the unpredicted influence of external agencies, something more is needed. The controlling system C must *receive information from the field* about the interval XY , and use it to control (wholly or partly) the selection of what E shall do next. This information is represented in Fig. 1 as carried to C by a 'mismatch signal' from a receptor system R coupled with the field through any number of modalities.

(ii) *Improvement in performance.* On the very simplest system, only the existence of the interval need be signalled to C . C must then be designed to keep E in randomly or systematically varied activity until the interval is reduced below some acceptable level (the 'Homeostat' of Ashby (1952) is a typical example).

We could generally achieve greater speed and efficiency, however (at the cost of greater complexity in the mismatch-signalling system), if the direction—and still better, the

magnitude—of the interval XY , and not merely its existence, were signalled back to C . In the extreme case we could provide C with a complete representation of the current state of the field of activity, from which to calculate automatically the best course of action for E .

If information can also be *stored* in C , its calculations can be extended to cover rates of movement and to make predictions on a statistical basis, so increasing still further the effectiveness of E 's activity. For this purpose we do not need to picture a discrete store of precisely symbolized information as in a digital computer. 'Storage' here means any modification of state due to information received and capable of influencing later activity, or however short a time.

(iii) *The distinction between goal-oriented and goal-guided activity.* In passing we may note (see MacKay, 1952) that there are some physical systems in which the requirements of goal-directed activity (in one sense) seem superficially to be met without any of the features of Fig. 1. A steel ball, for example, will 'pursue' a moving magnet and resist attempts to deflect it.

But in all systems of this sort, the essential difference is that *no other* activity is physically possible to the active agent in the given circumstances. The question of 'guidance' or 'direction', in the sense of a choice among physically open alternatives, does not arise. The required activity is in a sense uniquely implicit in the physical description of the situation requiring it. This is even more readily seen if we consider a physically equivalent model in which the ball is attached to the magnet by an elastic string. Its actions are merely 'newtonian' reactions to the situation in which it is placed.

It would, in fact, be rather tendentious to describe the magnet as the 'goal' of the ball. But if we must use the term, it might be better to avoid confusion by describing such activity as 'goal-oriented' rather than 'goal-directed'.

(iv) *Examples of goal-guided activity.* The simple flow map of Fig. 1 may be taken as typical of all situations in which goal-guided activity is required. In the simple thermostat, for example, the effector E is a heating unit. The field of activity F is the scale of temperature, on which the present state of the system is represented by some point Y , and the goal state by some pre-assigned point X . Information as to the existence and the direction (and sometimes the magnitude) of the interval XY is provided by some kind of thermometer (combining the functions of R and C) which serves to *compare* the present temperature with the goal temperature, and mechanically adjusts the activity of the heating unit in a direction calculated to reduce the discrepancy. In more complex cases the effector E may have many degrees of freedom (for example, it may have the equivalent of a large number of independent muscles), and its field of activity F will then be represented not by a one-dimensional line but by an abstract multi-dimensional space with a corresponding number of dimensions. In this space, however, the present state and the goal state will still be represented by points, X and Y , whose co-ordinates will be the values of the corresponding parameters. The magnitude and direction of the interval XY will have the same significance as in our one-dimensional illustration, and the system will work on exactly the same principles.

It will be clear from this example that in general the points X and Y in Fig. 1 do not represent physical *objects* in the environment of A . The point Y is an abstract way of representing the current *state* of the total situation, system plus environment. The point X similarly represents the state of the total situation towards which as goal the activity is guided.

By defining X and Y in this way as representing states and not physical objects, we can make the same formal description apply equally well to a self-guided missile chasing an aircraft, and a man chasing the solution to a crossword puzzle.

(v) *Towards inventive behaviour.* On the basic principle of Fig. 1, there is no difficulty in envisaging an automaton capable of interacting actively and apparently purposefully with its environment and in particular with human beings, in any field of activity for which it has appropriate effectors and receptors, and an adequate symbolic calculus embodied in its controlling element.

But there, of course, is the rub. It is true that human beings are educated in childhood to use a standard terminology, embodying standard categories of perception and description. To this extent we might be entitled to preform the categories in terms of which our model automaton structures its information. But human beings do more than carry out logically deductive operations in a fixed calculus. They invent new categories, and novel hypotheses, to meet an unforeseen flux of events. They imagine unrealized situations. . . .

How much farther can we go with our automaton? What kind of automaton, if any, could evolve for itself new descriptive categories appropriate to situations that its designer had not envisaged—and do it in a way analogous to the way in which a human being does? What place can we find in the theory of automata for such human activities as the invention of relevant and fruitful hypotheses, or the contemplation of imaginary situations?

In what follows I want to suggest, very tentatively, some principles on which a self-organizing system could be devised to parallel many of these inventive features of human activity—principles which at least bear realistic comparison with those so far recognized by psychologists in the development and functioning of the human information flow system.

III. ORGANIZING THE TRIAL PROCESS

(i) *Simple groping.* We have already seen that the system of Fig. 1 could show goal-guided activity even with the minimum of information about the interval XY . In the limit, it could simply run through its repertoire of activity until the mismatch signal disappeared. But if its repertoire is at all large, this process takes time. If the effector system E has l independent degrees of freedom, each associated with m alternatives, the number of combinations which might be tried, and hence the average time of hunting, rises as m^l . Thus this groping automaton cannot hope to cope with a complex situation in which the goal-state changes rapidly and unpredictably. Under such conditions it would do little worse if the mismatch signal were dispensed with altogether,* since even if by chance the current goal state were reached and XY reduced momentarily to zero, it would probably not remain so for more than a small fraction of the time spent in hunting.

(ii) *Directional guidance.* Things would be much better if the mismatch signal could indicate the *direction* of the interval XY . Even if only the *sign* of each component of the vector interval were signalled, the improvement could be enormous. Each trial (for an l -dimensional interval) would then provide approximately l independent yes-no decisions or (in the jargon of information theory) 'bits of information'. This could ideally reduce the minimum number of trials to only $\log_2 m$, i.e. $(1/l) (\log_2 m^l)$.

* The Irishman's clock, we remember (which had stopped), succeeded in telling the time correctly twice a day.

But with a complex effector system having many degrees of freedom, even this information capacity may be too much to ask of the mismatch-signalling system. This is not the place for a discussion of the informational requirements in detail; a start has been made in another paper (MacKay, 1955). But we must evidently ask at this stage whether there is no way of improving the performance of C without demanding from the mismatch signal the full selective-information content of $l \log_2 m$ bits which is ideally required in order to select the correct adaptive response by E to each fresh movement of X .

(iii) *Making use of 'redundancy'*. Information theory assures us that in principle there should be such a way if the movements of X show any regularity in their statistical structure, or 'redundancy', as it is called. In that case, as in an ordinary communication channel, the average selective-information content of the correct response may be much

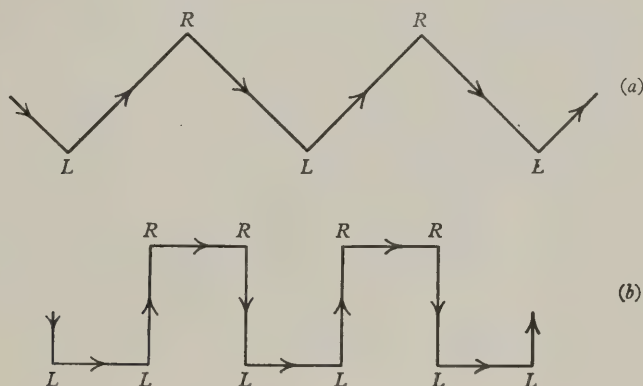


Fig. 2. Two typically 'redundant' sequences of bends in a road.

less than the $l \log_2 m$ bits which we first estimated. In practical terms, the trial process can afford to concentrate more on trying some combinations of activity rather than others; so on the average it can *save time*, or can reach the goal *in the same time with less information* from the mismatch-signalling system. Our problem is then to devise a way of profiting from the redundancy of the information sequence represented by the movements of the point X , or equivalently by the corresponding successful configurations of the activity of E .

(iv) *A simple example*. To take a simple example which I have used elsewhere (MacKay, 1955), imagine an automaton designed to run along a road consisting simply of a succession of straight runs and right-angle bends. The automaton has only two control buttons, L and R , which when pressed cause it to turn left or right respectively. When it reaches a bend in the road, a simple mismatch signal automatically induces random trials of L or R until the correct button is pressed and it moves on.

Now suppose that over a long stretch of road, L and R bends form a regular sequence, such as $LLLR$ (Fig. 2a), which we may call S_1 , or $LLRLLRR$ (Fig. 2b) (S_2). We could obviously save hunting time if we were now to install in the controlling system a simple button-pressing mechanism or 'organizer' which imitated the appropriate rhythmic sequence of bends in the road. For example, in the first case, this organizer would always respond to the mismatch signal by trying R if L had last been used, and vice versa. Thus every mismatch signal would at once induce the correct adaptive response without any

hunting. The high degree of redundancy in the series of bends permits of a correspondingly low information content per mismatch signal.

(v) *Second-order control.* Consider now a more complex road comprising a mixture of sequences of the forms S_1 and S_2 . Each time the sequence changes the internal imitative organizer must now make a corresponding change in the rhythm of control signals. Either its connexions must be altered, or its place must be taken by another mechanism suitably connected to generate the next rhythm.

We need, in fact, a second control mechanism, C_2 (Fig. 3), to organize trial routines from the repertoire of C_1 in the same way as C_1 organizes trial routines from the repertoire of E .

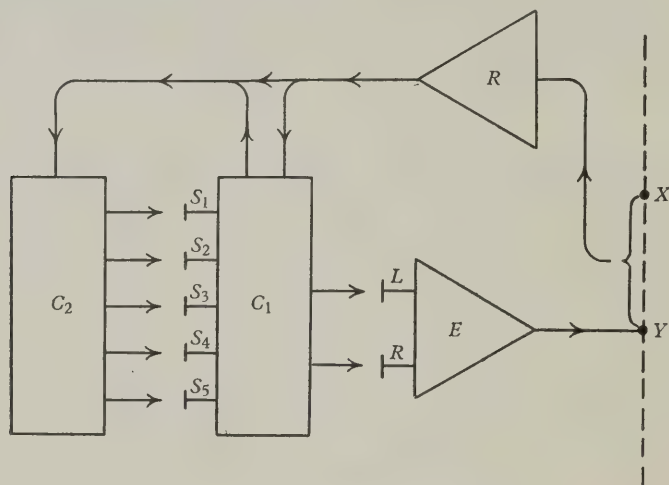


Fig. 3. The organization of control sequences.

If, for example, the road sequence changed from S_1 to S_2 and back again to S_1 in an alternating series, the organizer C_2 could be arranged to hunt through the repertoire of C_1 until it found the correct successor, each time that a succession of mismatch signals indicated that the road sequence had changed. But here again the efficiency could be increased, if C_2 incorporated a second-order button-pressing mechanism designed to try S_2 after S_1 and S_1 after S_2 .

Obviously we can envisage an indefinite hierarchy of such imitative organizers. Each will represent by its activity an *abstraction* descriptive of some aspect of the series of bends in the road.

IV. A STATISTICAL SELF-ORGANIZING SYSTEM

(i) *The automatic development of organization.* Our last example was so far all very well; but it presupposed that we, the designers, foresaw in advance the range of possible shapes of the road. Our next step—the fundamental one for our present purpose—must be to devise some principle on which the system could discover for itself the statistically stationary features of its environment, and develop its own characteristic organizing sequences as a result of its experience, starting if necessary from scratch.

(ii) *Statistically adjustable links.* The most promising way of solving this problem—the problem of growing an organizing system from scratch—would seem to be to design the

control mechanism out of statistically adjustable elements. By this I mean that instead of making its interconnexions hard and fast by means of switches that are either open or shut, we use 'statistical' interconnexions or links which have an adjustable *probability* of transferring information. This gives us all the flexibility we need. Since the 'thresholds' governing these probabilities may be continuously variable, the statistical structure of the activity pattern is capable of assuming an indefinite number of finely graded configurations, far larger than the number of combinations possible with simple on-off switches in each link. The information content represented by the current state of a given number of elements is correspondingly greater than in the simple case, so that the design can in principle be more economical of elements for a given required variability of activity pattern.

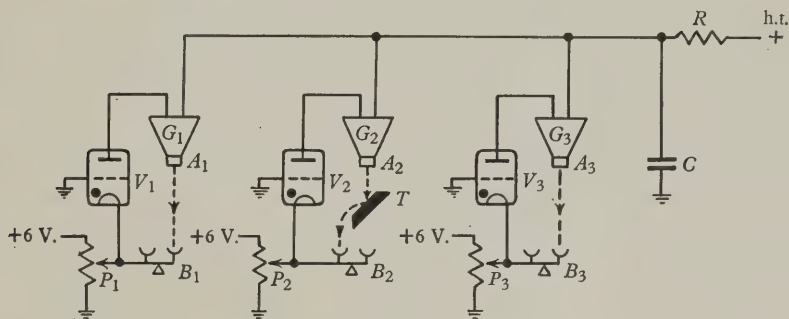


Fig. 4. A simple electronic device illustrating a statistical principle of self-organization.

(iii) *Control of statistical bias.* We can now envisage a self-organizing system in which the mismatch signal automatically adjusts the configuration of thresholds just mentioned, governing the statistical pattern of activity according to the success or failure achieved, so that successful sub-routines of activity become more likely to be tried again in similar circumstances, while unsuccessful ones become less likely to be tried.

Provided that the environmental world of activity shows consistent features, these could then in time come to be reflected in the relative probabilities of different organizing sub-routines.

(iv) *A simple example of a statistically self-organizing system.* An excessively simple illustration of this kind of process is provided by the automaton of Fig. 4. This device was built a few years ago to illustrate a lecture (MacKay, 1949). It has of course no pretensions to animal status, and justifies its existence solely as an explanatory device. It is made up of three identical units (the number is unrestricted in principle), each comprising thyatron valve V which can actuate an electromagnetic gate G so as to drop a ball-bearing from an aperture A over a two-pan balance B . B is mechanically linked to a potentiometer P , which governs the bias on thyatron V . The anodes of all thyratrons are supplied from a common capacitor C charged through a resistor R .

If now C is allowed to charge slowly, the thyatron with the lowest effective bias, say V_1 , will tend to fire first, discharging C and releasing a ball from A_1 . Normally this will fall into the right-hand pan of B , causing P_1 to rotate and increase the bias on V_1 . Thus the normal effect of a thyatron's firing is to reduce the probability of its firing next time.

If, however, a target T is placed under A_1 so as to deflect any balls from A_1 into the left-hand pan of B_1 , the opposite effect will occur. The success of the automaton in striking the target when V_1 fires will increase the probability that it will use V_1 next time.

Evidently no matter how the biases on different V 's are set initially, the automaton will tend to an equilibrium state in which only the thyatron V_1 fires and T is always hit. If, moreover, we move T to a new position, say no. 2, the automaton will gradually 'unlearn' its original 'set' and eventually will be found firing only V_2 .

In this example randomness is introduced only by the fluctuations of the thresholds of the various thyatrons, and the mismatch signals are quantized and very simple. Even so it illustrates the basic principle, whereby an automaton can selectively 'grow' modes of activity according to its experience of their relative success, and diminish by a kind of natural selection the probabilities of all except those which have been successful often enough.

(v) *Higher-order statistical control.* When we come to apply this principle of statistical control to the system of Fig. 3, the situation is rather more complex. The actions whose probabilities are adjustable are not merely external effector actions, but actions of one element on another; in particular, they include actions which alter 'conditional probabilities' of action by other elements. For example, in order to set up the sub-routine *LRLR* it is necessary that the action L shall modify the conditional probability, that R shall follow if and when the mismatch-signal next calls for action. There will thus be some situations of the general type of Fig. 1, wholly *within* the mechanism itself.

But the principle on which this system should be made self-organizing is the same. As before, we introduce a second organizer, C_2 , working now on the same statistical principles as C_1 which we have just discussed. Metaphorically speaking, C_2 can be on the watch for any regularities in the series of different routines found successful by C_1 . As these regularities develop, C_2 will spontaneously begin to grow patterns of activity to imitate them and pre-organize the probabilities of their occurrence, so reducing the average time spent in hunting through C_1 's repertoire, and at the same time increasing the statistical stability of any closed cycles that C_1 finds to be adaptive. If the organizing mechanism begins with most or all thresholds fairly low, so that all kinds of sequence are possible, then after a long series of *LRLR* bends in the road the probabilities of actions not contributing successfully to the trial process can be steadily reduced and the others reinforced until a firm pattern of organization emerges.

In fact we can envisage in this case, just as in paragraph III(v), a whole hierarchy of such organizers, whose activity has here developed from scratch a statistical pattern to match the stable structure discovered in the environmental sequence of events. Although the term 'hierarchy' suggests an ordered system of separate levels, the most flexible system would allow of activity from different levels to be mixed in each. We might even envisage an abstractive 'level' which took as its data the activity of the whole system of levels; and it may be profitable to investigate the results of allowing the activity of the 'top level' of the hierarchy to become the data for the 'bottom level', the mismatch signal being distributed initially more or less evenly throughout all levels. But the notion of a simple hierarchy is sufficient for our present purposes.

(vi) *The stabilizing principle.* Thus far we have spoken as if thresholds (and their corresponding 'transition probabilities') could be altered only by the action of the mismatch signal; as if 'adaptive success' were the only condition on which the probability of a given sequence should be increased. But there are several ways in which the adaptive efficiency

of the system (in a normal human environment) could be augmented, thereby incidentally increasing its resemblances to the human organism.

Let us introduce a second principle of organization, according to which the probability of a given transition is augmented automatically (though not indefinitely) according to the frequency of its occurrence, in addition to any modification resulting from its success or failure. What effect will this have on the growth of organization? First, and obviously, it will mean that every time a routine of activity comes 'back to where it started', it will receive some reinforcement in addition to whatever evaluation, positive or negative, is deduced from the mismatch signal. Closed cycles of activity will tend to be statistically favoured. As time goes on the adaptive organizing responses most likely to be tried will be those closed cycles which appear to be statistically nearest to what is required, as deduced from the mismatch signal. The system, in other words, will have a tendency to construct its imitative organizing sequences out of the nearest approximate combination of closed cycles of activity; the system will tend to perceive its environment as a com-
presence of associated 'wholes'.

The second effect will be to increase the speed with which the organization will grow in a relatively stable environment where the same sub-routine may be repeatedly called for. Closed sub-routines once found successful are less liable to disruption in subsequent trial processes, and more likely to be retained, excited or suppressed as wholes, ready more rapidly for use when the corresponding stable feature recurs in the flux of environmental events.

A third result of applying this principle is that the shorter closed cycles of organizing activity will tend to be even more favoured than they would be in any case. The effect is in fact analogous to 'negative resistance' or 'positive feedback', in the jargon of control theory; but it is feedback exerted on the probability pattern rather than on the activity itself. It tends to favour the formation of metastable 'eigenstates' of organizing activity and readiness for activity. The bias of our system will be towards conceptual simplicity. The mark of Occam's razor will appear naturally on the pattern of its activity.

(vii) *The 'anti-monopoly' principle.* The third principle of organization of the threshold control mechanism that should be mentioned here might seem superficially antagonistic to our earlier requirements. It is a provision that the intensity of a given pattern of activity will tend to die away with time. This we might describe as an 'anti-monopoly' proviso. Its function is to preserve the exploratory character of adaptive activity by ensuring that the system does not settle down for too long in any one adaptive mode. It thus increases the number of features of the environment likely within a given time to find themselves represented in the activity pattern.

This feature, analogous in some ways to the familiar 'adaptation' of sensory organs, is actually in no way antagonistic to our second requirement that the *transition* probabilities for closed cycles should automatically rise with time. To take a simple example, both requirements would be met in a system based on the principle of Fig. 4 if the high-tension supply battery were subject to polarization, so that its e.m.f. dropped steadily as long as current was drawn from it, and recovered only after a period of rest. In Fig. 4, the *relative* probability of the successful thyatron's firing increases with each successful action. It is only the probability of its *action in a given time interval* that decreases with each action.

In general, then, we must distinguish clearly between the control of relative probabilities of alternative courses of action, and the control of the probability of a given action in

given circumstances within a given time interval. The first determines the probability that, if an action occurs, it shall be of a given form. The second determines the probability that an action of this form shall occur (in a given time interval). Since the thresholds governing these probabilities are continually changing the probabilities cannot of course be interpreted as frequencies in an observable time series of actions, but (as in analogous situations in physics) only as the relative proportions, or frequencies, in an imaginary 'ensemble' or population of similar situations.

V. THE CONTROL OF DIMENSIONALITY

(i) *The speed of development.* By intelligent use of the foregoing principles, I think that a self-organizing system can be envisaged which could develop, *ab initio*, adaptive powers comparable with those of a human being, if subjected to a similar course of typical experience. But we have skirted one important practical question. Even with all our aids to efficiency, is there any hope of this development taking place in a *time* comparable with that required by an average human infant? Will not the number of possibilities to be eliminated, and the number of wasted trials, be astronomically large if the field of activity is to be as complex as the human one?

(ii) *Statistical control in the effector system.* I have only a tentative answer to this question, but it is one that seems promising. We have argued hitherto as if the degrees of freedom of the effector system *E* were large in number and fixed, so that each step in the process of development amounted to the elimination of all but one of a vast number of alternatives.

Let us picture now, however, an effector system constructed on much the same statistical lines as the control system we have discussed; and suppose that initially the elements are so tightly coupled together (by statistical links with low thresholds) that the effective number of independent degrees of freedom is very small. The responses possible will be relatively few and crude, but the initial trial and error processes of goal pursuit can be correspondingly rapid. Few effector actions will be highly successful, but the best in given circumstances will be fairly quickly discovered. The broad outlines of the statistical structure of the environmental events will begin fairly quickly to be reflected in the pattern of internal organizing activity.

Suppose now that as this happens, the degrees of freedom of the effector system are gradually and correspondingly increased in number, by a reduction in the statistical coupling between elements under the guidance of the mismatch signal. To take a simple example, the system might start with the controls of all of five 'fingers' interconnected so as to function as a single unit. As the organizing process went on, it could be arranged that the general level of the thresholds in the interconnecting links should rise, so that the 'fingers' would begin to gain independence; but any links found in practice to promote successful responses could be selectively favoured, and could retain a greater-than-average probability of functioning.

(iii) *Comparison with the human system.* In a suitable environment offering a sufficiently typical sample of the regularities normally to be encountered, it seems not unreasonable (as I have argued elsewhere (MacKay, 1955)) that the organizing system should now be able to grow in complexity in the ways we have discussed, step by step with the effector system, at a rate comparable with that of the learning process in human infancy. At least

the system may perhaps bear realistic comparison with the human one, if only in order that the differences as they emerge may lead to its refinement.

But at present we are concerned not with the comparison so much as with the fact that this kind of activity is possible in a self-organizing information-flow system, and with its significance as one solution to the problem of concept formation.

For it is indeed in the same principles of organization which we have hitherto treated as a means of improving goal pursuit, that we shall find a possible solution to the problems of inventive activity—particularly concept formation—which are our chief concern.

VI. THE REPRESENTATION OF THE FIELD OF ACTIVITY

(i) *The symbolization of information.* In a digital computer the symbols for concepts are patterns of 1's and 0's or their equivalents such as electrical dots and dashes. In an analogue computer such as a guided missile might use, abstractions such as 'distance of target' are represented by the magnitudes of voltages or currents. What corresponds to these symbols in our present system? How is information about its field of action represented internally?

(ii) *Pre-conceptual information.* At the first stage, the stage of *reception*, it is represented by what we have called the mismatch signal, which may include, for instance, electrical stimuli from visual receptors. Much of this information may be coupled directly to conditional-probability controls associated with actions which in a human being we should describe as reflexes. Incoming information then leads to a conditioned or unconditioned response without any necessary intermediate stage of conceptualization.

In human beings such activities range from visceral responses of which we may be unaware, through such reflexes as swallowing which we can sometimes control, to learned patterns such as swimming or cycling which may have formed under conscious guidance. All are characterized by the fact that we 'don't think about them'. We say we 'know how to' cycle, etc., using 'know' in a different sense from that which we use for conceptualized 'facts'.

(iii) *Symbolization in terms of organizing activity.* But in our information-flow model there is a second sort of internal activity which logically 'represents' features of the world of activity. This is the activity of the internal organizing system. Its imitative sub-routines, when evoked in order to match successfully some change in the world of activity, form an active symbol for that change. What is more, since most sub-routines will tend to form out of standard sequences, this symbol will amount logically to a description in terms of the corresponding standard categories. In normal circumstances, then, there will be in the organizing activity a running representation of those features of the field of action to which the system as a whole is currently matching its own activity, internal and external.

In its 'visual' system, for example, we may picture the internal activity as organized to match (in a sense, to cancel out actively) the incoming visual signals. It will do this as a result of combinations of sub-routines of organizing activity which will have developed through adaptive success in the past, and will amount therefore to a representation of the visual field in terms of previously exemplified categories.

(iv) *The correlate of perception.* Once again the details cannot detain us here. But the essential point is that the symbols constituted by the internally evoked matching response

(the organizing activity) seem to be related to the incoming recepta from the sensory organs of the information-flow model in much the same way as what we call *percepts* are related to the recepta from human sensory organs.

Among other points worthy of note, we see that (a) the description offered by this symbolic representation is selective: certain features are 'noticed' and others ignored; (b) the description possible at a given level is not unique, since several combinations may 'match' equally well; there will be illusions and 'ambiguous figures' for this system just as for human observers (e.g. the Ames demonstrations (Ames, 1946)); (c) complementary descriptions may be constituted by organizing activity at different levels of abstraction (e.g. an illuminated advertizing sign may be perceived either as a number of electric lamps or as the sequence of words delineated by them); and (d) the state of readiness both for further effector action and for further internal organizing activity depends on the pattern of these symbols, in the same logical way as our human state of readiness depends on what we perceive in our world of activity.

These are only a few of many points in favour of attributing to the organizing sub-routines the role of symbols for percepts.

(v) *Imagination and thinking*. But we must go further. What we know is not limited to what we currently perceive. We know 'what is the case'—or believe we do—even after we have shut our eyes, or gone away for a year from a familiar landscape. Operationally we mean by this, among other things, (a) that we can 'evoke mental images' of the landscape, or of the relationship (concrete or abstract) which we claim to know; (b) that in all appropriate circumstances we are organized to react to the pattern of events which should come about if 'what we believe' is the case. For example, we move so as to avoid the chair which we believe to stand in the middle of a dark room. Or we (consciously or unconsciously) select the statements we make to a friend so as to avoid logical contradiction with a proposition we believe.

Now the model shows certain features of its activity that seem closely parallel to these. In particular, it is possible and would indeed be normal in the absence of excessive external stimulation, for some of the internal organizing mechanisms to 'run free' without evoking the corresponding effector activity. All that is necessary is a flexible inhibitory system under the general control of the organizers, so that they can suppress, in whole or in part, the effector action normally induced by the free running activity.

When this happens, long trains of organizing activity will normally be excited through the statistical couplings formed as a result of past 'experience', and internal goal-guided activity can take place, in which the goal pursued is *abstractly defined in terms of the degrees of freedom of the organizing system*, in the same way as the goal *X* (Fig. 1) was defined in terms of those of the effector system *E*. Along such lines it seems possible to find correlates of many of the human activities we group under the names of *thinking* and *imagining*.

Hypotheses, for example, are operationally symbolized by sub-routines which organize readiesses for the corresponding features that should be shown by the world of activity if the hypotheses were true. A familiar scene can be 'imagined' by re-evoking the pattern of internal organizing activity by which it was 'matched' originally. An unfamiliar scene, known only from a description, can be 'imagined' by allowing the internal activity symbolizing its description to evoke what it will by way of activity statistically associated with past instances of the descriptive terms. If the model has come to use organizing routines mainly from its visual system as symbols for concepts, such evoked activity may

amount to a symbolization of a complete visual image. If, however, some of the other types of organizing routines form its main conceptual vocabulary, the visual component of its 'imagined' situation may be small. All such 'imaginative' activity is distinguished from 'hallucination' by the fact that the mismatch signal will be present in its normal channels, indicating that what is symbolized is not a representation of the current state of the environment.

(vi) *Functional disorder*. This raises the whole question of functional disorder in the automaton, and its behavioural resemblances to human mental disorder. But the detailed consideration of this topic would lead too far and will be undertaken in further papers. (Some of the operational differences between normal imagination, illusion (e.g. optical), and hallucination, are discussed in an earlier paper (MacKay, 1952).) Suffice it to say that the behaviour accompanying differential threshold changes, and diversion or interruption of the mismatch signal (in various ways), adds up to a picture of which no major feature is without its known correlate in human psychopathology, and in which many of the major symptoms of mental disorder find natural analogues.

VII. CONCLUSION

And so we could go on, but we must bring this lengthy discussion to an end; for the outlines of the correlation possible between the model's internal activity and human concept-handling processes should now be clear. And perhaps enough has been said to indicate that such a self-organizing system shows some promise of usefulness as a kind of foil for psychological theories of human personality. Of each point of debate we may ask 'What difference would it make in the information-flow model?' or 'How does it sound when translated into terms of the information-flow pattern?' Needless to say, the information-flow model is likely to find itself refined as often as the psychological theories in consequence of such discipline; but it seems at least possible that the process may be a convergent one that could materially further psychological understanding.

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IS CONSCIOUSNESS A BRAIN PROCESS?

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The thesis that consciousness is a process in the brain is put forward as a reasonable scientific hypothesis, not to be dismissed on logical grounds alone. The conditions under which two sets of observations are treated as observations of the same process, rather than as observations of two independent correlated processes, are discussed. It is suggested that we can identify consciousness with a given pattern of brain activity, if we can explain the subject's introspective observations by reference to the brain processes with which they are correlated. It is argued that the problem of providing a physiological explanation of introspective observations is made to seem more difficult than it really is by the 'phenomenological fallacy', the mistaken idea that descriptions of the appearances of things are descriptions of the actual state of affairs in a mysterious internal environment.

I. INTRODUCTION

The view that there exists a separate class of events, mental events, which cannot be described in terms of the concepts employed by the physical sciences no longer commands the universal and unquestioning acceptance amongst philosophers and psychologists which it once did. Modern physicalism, however, unlike the materialism of the seventeenth and eighteenth centuries, is behaviouristic. Consciousness on this view is either a special type of behaviour, 'sampling' or 'running-back-and-forth' behaviour as Tolman (1932, p. 206) has it, or a disposition to behave in a certain way, an itch for example being a temporary propensity to scratch. In the case of cognitive concepts like 'knowing', 'believing', 'understanding', 'remembering' and volitional concepts like 'wanting' and 'intending', there can be little doubt, I think, that an analysis in terms of dispositions to behave (Wittgenstein, 1953; Ryle, 1949) is fundamentally sound. On the other hand, there would seem to be an intractable residue of concepts clustering around the notions of consciousness, experience, sensation and mental imagery, where some sort of inner process story is unavoidable (Place, 1954). It is possible, of course, that a satisfactory behaviouristic account of this conceptual residuum will ultimately be found. For our present purposes, however, I shall assume that this cannot be done and that statements about pains and twinges, about how things look, sound and feel, about things dreamed of or pictured in the mind's eye, are statements referring to events and processes which are in some sense private or internal to the individual of whom they are predicated. The question I wish to raise is whether in making this assumption we are inevitably committed to a dualist position in which sensations and mental images form a separate category of processes over and above the physical and physiological processes with which they are known to be correlated. I shall argue that an acceptance of inner processes does not entail dualism and that the thesis that consciousness is a process in the brain cannot be dismissed on logical grounds.

II. THE 'IS' OF DEFINITION AND THE 'IS' OF COMPOSITION

I want to stress from the outset that in defending the thesis that consciousness is a process in the brain, I am not trying to argue that when we describe our dreams, fantasies and sensations we are talking about processes in our brains. That is, I am not claiming that

statements about sensations and mental images are reducible to or analysable into statements about brain processes, in the way in which 'cognition statements' are analysable into statements about behaviour. To say that statements about consciousness are statements about brain processes is manifestly false. This is shown (a) by the fact that you can describe your sensations and mental imagery without knowing anything about your brain processes or even that such things exist, (b) by the fact that statements about one's consciousness and statements about one's brain processes are verified in entirely different ways and (c) by the fact that there is nothing self-contradictory about the statement 'X has a pain but there is nothing going on in his brain'. What I do want to assert, however, is that the statement 'consciousness is a process in the brain', although not necessarily true, is not necessarily false. 'Consciousness is a process in the brain', on my view is neither self-contradictory nor self-evident; it is a reasonable scientific hypothesis, in the way that the statement 'lightning is a motion of electric charges' is a reasonable scientific hypothesis.

The all but universally accepted view that an assertion of identity between consciousness and brain processes can be ruled out on logical grounds alone, derives, I suspect, from a failure to distinguish between what we may call the 'is' of definition and the 'is' of composition. The distinction I have in mind here is the difference between the function of the word 'is' in statements like 'a square is an equilateral rectangle', 'red is a colour', 'to understand an instruction is to be able to act appropriately under the appropriate circumstances', and its function in statements like 'his table is an old packing case', 'her hat is a bundle of straw tied together with string', 'a cloud is a mass of water droplets or other particles in suspension'. These two types of 'is' statement have one thing in common. In both cases it makes sense to add the qualification 'and nothing else'. In this they differ from those statements in which the 'is' is an 'is' of predication; the statements 'Toby is 80 years old and nothing else', 'her hat is red and nothing else' or 'giraffes are tall and nothing else', for example, are nonsense. This logical feature may be described by saying that in both cases both the grammatical subject and the grammatical predicate are expressions which provide an adequate characterization of the state of affairs to which they both refer.

In another respect, however, the two groups of statements are strikingly different. Statements like 'a square is an equilateral rectangle' are necessary statements which are true by definition. Statements like 'his table is an old packing case', on the other hand, are contingent statements which have to be verified by observation. In the case of statements like 'a square is an equilateral rectangle' or 'red is a colour', there is a relationship between the meaning of the expression forming the grammatical predicate and the meaning of the expression forming the grammatical subject, such that whenever the subject expression is applicable the predicate must also be applicable. If you can describe something as red then you must also be able to describe it as coloured. In the case of statements like 'his table is an old packing case', on the other hand, there is no such relationship between the meanings of the expressions 'his table' and 'old packing case'; it merely so happens that in this case both expressions are applicable to and at the same time provide an adequate characterization of the same object. Those who contend that the statement 'consciousness is a brain process' is logically untenable base their claim, I suspect, on the mistaken assumption that if the meanings of two statements or expressions are quite unconnected, they cannot both provide an adequate characterization of the same object or state of affairs: if something is a state of consciousness, it cannot be a

brain process, since there is nothing self-contradictory in supposing that someone feels a pain when there is nothing happening inside his skull. By the same token we might be led to conclude that a table cannot be an old packing case, since there is nothing self-contradictory in supposing that someone has a table, but is not in possession of an old packing case.

III. THE LOGICAL INDEPENDENCE OF EXPRESSIONS AND THE ONTOLOGICAL INDEPENDENCE OF ENTITIES

There is, of course, an important difference between the table/packing case case and the consciousness/brain process case in that the statement 'his table is an old packing case' is a particular proposition which refers only to one particular case, whereas the statement 'consciousness is a process in the brain' is a general or universal proposition applying to all states of consciousness whatever. It is fairly clear, I think, that if we lived in a world in which all tables without exception were packing cases, the concepts of 'table' and 'packing case' in our language would not have their present logically independent status. In such a world a table would be a species of packing case in much the same way that red is a species of colour. It seems to be a rule of language that whenever a given variety of object or state of affairs has two characteristics or sets of characteristics, one of which is unique to the variety of object or state of affairs in question, the expression used to refer to the characteristic or set of characteristics which defines the variety of object or state of affairs in question will always entail the expression used to refer to the other characteristic or set of characteristics. If this rule admitted of no exception it would follow that any expression which is logically independent of another expression which uniquely characterizes a given variety of object or state of affairs, must refer to a characteristic or set of characteristics which is not normally or necessarily associated with the object or state of affairs in question. It is because this rule applies almost universally, I suggest, that we are normally justified in arguing from the logical independence of two expressions to the ontological independence of the states of affairs to which they refer. This would explain both the undoubted force of the argument that consciousness and brain processes must be independent entities because the expressions used to refer to them are logically independent and, in general, the curious phenomenon whereby questions about the furniture of the universe are often fought and not infrequently decided merely on a point of logic.

The argument from the logical independence of two expressions to the ontological independence of the entities to which they refer breaks down in the case of brain processes and consciousness, I believe, because this is one of a relatively small number of cases where the rule stated above does not apply. These exceptions are to be found, I suggest, in those cases where the operations which have to be performed in order to verify the presence of the two sets of characteristics inhering in the object or state of affairs in question can seldom if ever be performed simultaneously. A good example here is the case of the cloud and the mass of droplets or other particles in suspension. A cloud is a large semi-transparent mass with a fleecy texture suspended in the atmosphere whose shape is subject to continual and kaleidoscopic change. When observed at close quarters, however, it is found to consist of a mass of tiny particles, usually water droplets, in continuous motion. On the basis of this second observation we conclude that a cloud is a mass of tiny particles and nothing else. But there is no logical connexion in our language between a cloud and a mass of tiny particles; there is nothing self-contradictory in talking about a cloud which is not com-

posed of tiny particles in suspension. There is no contradiction involved in supposing that clouds consist of a dense mass of fibrous tissue; indeed, such a consistency seems to be implied by many of the functions performed by clouds in fairy stories and mythology. It is clear from this that the terms 'cloud' and 'mass of tiny particles in suspension' mean quite different things. Yet we do not conclude from this that there must be two things, the mass of particles in suspension and the cloud. The reason for this, I suggest, is that although the characteristics of being a cloud and being a mass of tiny particles in suspension are invariably associated, we never make the observations necessary to verify the statement 'that is a cloud' and those necessary to verify the statement 'this is a mass of tiny particles in suspension' at one and the same time. We can observe the micro-structure of a cloud only when we are enveloped by it, a condition which effectively prevents us from observing those characteristics which from a distance lead us to describe it as a cloud. Indeed, so disparate are these two experiences that we use different words to describe them. That which is a cloud when we observe it from a distance becomes a fog or mist when we are enveloped by it.

IV. WHEN ARE TWO SETS OF OBSERVATIONS OBSERVATIONS OF THE SAME EVENT?

The example of the cloud and the mass of tiny particles in suspension was chosen because it is one of the few cases of a general proposition involving what I have called the 'is' of composition which does not involve us in scientific technicalities. It is useful because it brings out the connexion between the ordinary everyday cases of the 'is' of composition like the table/packing case example and the more technical cases like 'lightning is a motion of electric charges' where the analogy with the consciousness/brain process case is most marked. The limitation of the cloud/tiny particles in suspension case is that it does not bring out sufficiently clearly the crucial problem of how the identity of the states of affairs referred to by the two expressions is established. In the cloud case the fact that something is a cloud and the fact that something is a mass of tiny particles in suspension are both verified by the normal processes of visual observation. It is arguable, moreover, that the identity of the entities referred to by the two expressions is established by the continuity between the two sets of observations as the observer moves towards or away from the cloud. In the case of brain processes and consciousness there is no such continuity between the two sets of observations involved. A closer introspective scrutiny will never reveal the passage of nerve impulses over a thousand synapses in the way that a closer scrutiny of a cloud will reveal a mass of tiny particles in suspension. The operations required to verify statements about consciousness and statements about brain processes are fundamentally different.

To find a parallel for this feature we must examine other cases where an identity is asserted between something whose occurrence is verified by the ordinary processes of observation and something whose occurrence is established by special scientific procedures. For this purpose I have chosen the case where we say that lightning is a motion of electric charges. As in the case of consciousness, however closely we scrutinize the lightning we shall never be able to observe the electric charges, and just as the operations for determining the nature of one's state of consciousness are radically different from those involved in determining the nature of one's brain processes, so the operations for determining the occurrence of lightning are radically different from those involved in determining the occurrence of a motion of electric charges. What is it, therefore, that leads us to say that

the two sets of observations are observations of the same event? It cannot be merely the fact that the two sets of observations are systematically correlated such that whenever there is lightning there is always a motion of electric charges. There are innumerable cases of such correlations where we have no temptation to say that the two sets of observations are observations of the same event. There is a systematic correlation, for example, between the movement of the tides and the stages of the moon, but this does not lead us to say that records of tidal levels are records of the moon's stages or vice versa. We speak rather of a causal connexion between two independent events or processes.

The answer here seems to be that we treat the two sets of observations as observations of the same event, in those cases where the technical scientific observations set in the context of the appropriate body of scientific theory provide an immediate explanation of the observations made by the man in the street. Thus we conclude that lightning is nothing more than a motion of electric charges, because we know that a motion of electric charges through the atmosphere, such as occurs when lightning is reported, gives rise to the type of visual stimulation which would lead an observer to report a flash of lightning. In the moon/tide case, on the other hand, there is no such direct causal connexion between the stages of the moon and the observations made by the man who measures the height of the tide. The causal connexion is between the moon and the tides, not between the moon and the measurement of the tides.

V. THE PHYSIOLOGICAL EXPLANATION OF INTROSPECTION AND THE PHENOMENOLOGICAL FALLACY

If this account is correct, it should follow that in order to establish the identity of consciousness and certain processes in the brain, it would be necessary to show that the introspective observations reported by the subject can be accounted for in terms of processes which are known to have occurred in his brain. In the light of this suggestion it is extremely interesting to find that when a physiologist as distinct from a philosopher finds it difficult to see how consciousness could be a process in the brain, what worries him is not any supposed self-contradiction involved in such an assumption, but the apparent impossibility of accounting for the reports given by the subject of his conscious processes in terms of the known properties of the central nervous system. Sir Charles Sherrington has posed the problem as follows: 'The chain of events stretching from the sun's radiation entering the eye to, on the one hand, the contraction of the pupillary muscles, and on the other, to the electrical disturbances in the brain-cortex are all straightforward steps in a sequence of physical "causation", such as, thanks to science, are intelligible. But in the second serial chain there follows on, or attends, the stage of brain-cortex reaction an event or set of events quite inexplicable to us, which both as to themselves and as to the causal tie between them and what preceded them science does not help us; a set of events seemingly incommensurable with any of the events leading up to it. The self "sees" the sun; it senses a two-dimensional disc of brightness, located in the "sky", this last a field of lesser brightness, and overhead shaped as a rather flattened dome, coping the self and a hundred other visual things as well. Of hint that this is within the head there is none. Vision is saturated with this strange property called "projection", the unargued inference that what it sees is at a "distance" from the seeing "self". Enough has been said to stress that in the sequence of events a step is reached where a physical situation in the

brain leads to a psychical, which however contains no hint of the brain or any other bodily part. . . . The supposition has to be, it would seem, two continuous series of events, one physico-chemical, the other psychical, and at times interaction between them' (Sherrington, 1947, pp. xx-xxi).

Just as the physiologist is not likely to be impressed by the philosopher's contention that there is some self-contradiction involved in supposing consciousness to be a brain process, so the philosopher is unlikely to be impressed by the considerations which lead Sherrington to conclude that there are two sets of events, one physico-chemical, the other psychical. Sherrington's argument for all its emotional appeal depends on a fairly simple logical mistake, which is unfortunately all too frequently made by psychologists and physiologists and not infrequently in the past by the philosophers themselves. This logical mistake, which I shall refer to as the 'phenomenological fallacy', is the mistake of supposing that when the subject describes his experience, when he describes how things look, sound, smell, taste or feel to him, he is describing the literal properties of objects and events on a peculiar sort of internal cinema or television screen, usually referred to in the modern psychological literature as the 'phenomenal field'. If we assume, for example, that when a subject reports a green after-image he is asserting the occurrence inside himself of an object which is literally green, it is clear that we have on our hands an entity for which there is no place in the world of physics. In the case of the green after-image there is no green object in the subject's environment corresponding to the description that he gives. Nor is there anything green in his brain; certainly there is nothing which could have emerged when he reported the appearance of the green after-image. Brain processes are not the sort of things to which colour concepts can be properly applied.

The phenomenological fallacy on which this argument is based depends on the mistaken assumption that because our ability to describe things in our environment depends on our consciousness of them, our descriptions of things are primarily descriptions of our conscious experience and only secondarily, indirectly and inferentially descriptions of the objects and events in our environments. It is assumed that because we recognize things in our environment by their look, sound, smell, taste and feel, we begin by describing their phenomenal properties, i.e. the properties of the looks, sounds, smells, tastes and feels which they produce in us, and infer their real properties from their phenomenal properties. In fact, the reverse is the case. We begin by learning to recognize the real properties of things in our environment. We learn to recognize them, of course, by their look, sound, smell, taste and feel; but this does not mean that we have to learn to describe the look, sound, smell, taste and feel of things before we can describe the things themselves. Indeed, it is only after we have learnt to describe the things in our environment that we can learn to describe our consciousness of them. We describe our conscious experience not in terms of the mythological 'phenomenal properties' which are supposed to inhere in the mythological 'objects' in the mythological 'phenomenal field', but by reference to the actual physical properties of the concrete physical objects, events and processes which normally, though not perhaps in the present instance, give rise to the sort of conscious experience which we are trying to describe. In other words when we describe the after-image as green, we are not saying that there is something, the after-image, which is green, we are saying that we are having the sort of experience which we normally have when, and which we have learnt to describe as, looking at a green patch of light.

Once we rid ourselves of the phenomenological fallacy we realize that the problem of explaining introspective observations in terms of brain processes is far from insuperable. We realize that there is nothing that the introspecting subject says about his conscious experiences which is inconsistent with anything the physiologist might want to say about the brain processes which cause him to describe the environment and his consciousness of that environment in the way he does. When the subject describes his experience by saying that a light which is in fact stationary, appears to move, all the physiologist or physiological psychologist has to do in order to explain the subject's introspective observations, is to show that the brain process which is causing the subject to describe his experience in this way, is the sort of process which normally occurs when he is observing an actual moving object and which therefore normally causes him to report the movement of an object in his environment. Once the mechanism whereby the individual describes what is going on in his environment has been worked out, all that is required to explain the individual's capacity to make introspective observations is an explanation of his ability to discriminate between those cases where his normal habits of verbal description are appropriate to the stimulus situation and those cases where they are not and an explanation of how and why, in those cases where the appropriateness of his normal descriptive habits is in doubt, he learns to issue his ordinary descriptive protocols preceded by a qualificatory phrase like 'it appears', 'seems', 'looks', 'feels', etc.

I am greatly indebted to my fellow-participants in a series of informal discussions on this topic which took place in the Department of Philosophy, University of Adelaide, in particular to Mr C. B. Martin for his persistent and searching criticism of my earlier attempts to defend the thesis that consciousness is a brain process, to Prof. D. A. T. Gasking, of the University of Melbourne, for clarifying many of the logical issues involved and to Prof. J. J. C. Smart for moral support and encouragement in what often seemed a lost cause.

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LISTENING BETWEEN AND DURING PRACTISED AUDITORY DISTRACTIONS

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Subjects were instructed to answer a rapid series of questions, and during the series a buzzer was sometimes sounded. Some subjects were instructed to make a reaction to this buzzer; this produced an interference with the speech task, both at the instant of the buzz and also between buzzes. The latter interference was reduced by previous practice at the buzzer reaction, but the former was not.

If the buzzer was one of two possible signals for a two-choice reaction, the interference at the moment of the buzz was greater, suggesting that the situation is most easily described in the terms of information theory. But previous practice in this case causes the subject to perform the secondary task in a way which requires less information and so interferes less. The effect of practice in 'automatizing' a task is thus real, but has important limitations.

I. INTRODUCTION

Human performance is often said to change with practice by becoming more 'automatic'. By this is meant, on the one hand, that a practised response is less likely to give rise to introspective reports; as was found, for example, by Solomons (1899) and Bryan & Harter (1897). On the other hand, it is usually said that an automatized habit interferes less with other simultaneous activity (James, 1890). These two characteristics may be essentially the same, since introspective report and bodily action can from one point of view be taken as different responses to the same stimulus. When both appear, the activity within the organism is clearly more widespread than when introspective report is lacking. Such a reduction in the number of responses to one stimulus may well involve an increased ability to make simultaneous responses to different stimuli. The present paper is therefore intended to examine the extent to which practice on one task may allow other tasks to be carried out at the same time.

The particular tasks used all involve hearing, since previous work with that sense (Broadbent, 1952) has shown interference, between tasks, which cannot be ascribed to difficulties of sensory adjustment. Results from the ear can therefore be extended to, say, the eye more confidently than in the reverse direction, though such an extension must be tentative until further evidence is available. The earlier work has also shown practice effects in the recognition of heard voices in a babel, which makes it desirable to use a similar technique when investigating practice more thoroughly. Furthermore, it is especially true of auditory tasks that everyday experience places great emphasis on the value of practice: those who operate communication systems in which several voices may be heard at once find that continued familiarity with the situation brings impressive subjective benefits. Johnston (1953) has recently pointed out that workers in a noisy factory may understand speech in that factory though not in other noisy situations.

II. METHOD

The plan was adopted of asking all subjects to carry out a task involving speech perception, on which a standard amount of practice was given. Additional tasks were then added to the central one, the nature and degree of familiarity of the additional task

varying with different groups of subjects; and the amount of interference produced in the central task was determined.

The central task was that used by Broadbent (1952). The subject observed a piece of paper marked in five divisions, each section carrying two abstract symbols (Fig. 1). The paper was changed from time to time. He heard messages consisting of the names of two symbols and the number of a position; if those two symbols agreed with the two shown in that position he said 'yes', and if not, 'no'. Each message was also prefixed by one of two

1	2	3	4	5
				
				

Fig. 1. The visual part of the task; the arrangement of the symbols was changed at every twelve messages. A typical message would be 'GDO Square Circle Three?' The appropriate answer would be 'GDO Yes' with the symbols as shown. See text for further explanation.

possible call signs, and the particular one used had to be repeated by the subject before his answer. These messages were delivered through a Grampian G. 129 loudspeaker mounted in front of the subject 3 ft. away and slightly above head-level, the speaker being fed from one channel of a Ferrograph two-channel tape recorder. All subjects received a standard practice period of 10 min. on this task on the day before the main experiment.

On the experimental day all subjects heard four runs, each of twenty-four questions separated by 2 sec. intervals so that each run lasted for approximately 1.3 min. This is a rather fast rate, resulting in an appreciable number of errors. A second loudspeaker, built into the Ferrograph case, was mounted on the subject's left-hand side 3 ft. away at shoulder height. This was fed from the second channel of the recorder, and as every fifth question was spoken through the central speaker, a buzzer sounded through the left-hand speaker, starting with the second word of the question and finishing at the end of the third. Thus this occurred four times in each run. The output levels of both channels were kept constant throughout the experiment; each gave 80 db. on a General Radio sound-level meter at the subject's position. The order in which the four records were heard was rotated within each group of subjects. All ninety-two subjects were naval ratings aged 30 or less. The majority were Fleet Air Arm ratings; in certain groups a few ratings of other trades were included, but a check on the scores obtained on the speech task in the standard practice session showed that this did not affect the comparability of the groups. No subject served in more than one group. There were eight groups in all, and of these the first three were the most important, i.e. groups NR, K and KP.

Each group had either ten or twelve subjects. The instructions to each group were as follows. Each group is identified by letters standing for the type of instructions received (see also Table 1):

Group NR (No Response) were instructed that a buzzer would sound from time to time, but that it was to be ignored.

Group K (Key) were instructed that when the buzzer sounded they were to press a key provided, 'without letting it interfere with answering the messages'.

Group KP (Key after Practice) received the same instructions as group K. On the previous day, however, they had heard a record of twenty buzzes similar to those used on the experimental day, separated on the average by 3 sec., and had been instructed to press the key whenever the buzzer sounded. No messages were presented while this practice was going on. On the experimental day another similar practice session immediately preceded the main session.

Group NR-K (No Response, then Key) received the same instructions as group NR on the first two runs, but the same as group K on the last two runs.

Group S-K (Stamp, then Key) received the same instructions as group K, save that the response required was a stamp of the foot during the first two runs. On the last two runs this was changed to the usual pressing of a key.

Group LP-K (Listening Practice, then Key) were treated similarly to group KP except for the content of their practice sessions. In each session they carried out a task in which two numbers were heard simultaneously on the two speakers. One of these numbers was always 1, although it might be on either speaker, and the task was to repeat the other number. Forty pairs of numbers were given in each session, the interval between pairs being 2 sec. This task was intended to give practice at selective listening without requiring the actual stimuli and responses used in the other tasks.

Group K/S (Key or Stamp) were instructed that when the buzzer sounded they were to press the key, provided the experimenter remained inactive. But if the experimenter laid his hand over the key as the buzzer sounded the subject was to stamp his foot instead. (The key was for all groups placed next to the visual display (Fig. 1) so as to be within the field of view; and for all groups the experimenter sat next to the key. But he remained inactive except in this group and group K/SP.) Two such extra stimuli were given in each run, allotted unsystematically between the four buzzes, although given at the same points to all subjects in the group.

Group K/SP (Key or Stamp after Practice) received the same instructions as group K/S but also had two practice sessions as in group KP. In each session half the buzzes at random were accompanied by the extra hand stimulus, and on these occasions the subject had to stamp his foot instead. Thus, like group KP, they practised the reaction actually required in the main session.

In all groups the subjects carried out the response to the buzzer as instructed; it appeared that they treated this task rather than the listening one as the most important.

III. ANALYSIS OF DATA

All scores used were in terms of errors in answering the questions. Two main classes of score may be distinguished: first, errors on questions which arrived at the same time as the buzzer sounded. These will be termed *buzz errors*. Secondly, errors on the questions

arriving between the times when the buzzer sounded. These will be termed *between-buzz* errors. In the latter class the questions immediately before and after buzzes furnish a few interesting comparisons, and will be referred to as *before* (or *after*) *buzz errors*.

As by the very nature of the problem only a few readings could be obtained from each subject on buzz errors, analysis of variance could not be used. Even in the more numerous between-buzz errors, Bartlett's test showed significant non-homogeneity of variance. Adequate tests were, however, devised as follows.

For all comparisons between different groups of subjects the tau technique of Whitfield (1947) was employed. For comparisons between two different scores of the same subject, such as practice effects, tau was inappropriate. In such cases the total score for each subject, including both conditions, was calculated and expressed as a proportion of the total possible. On the null hypothesis of zero difference between conditions, if

$$\text{this proportion} = p,$$

$$q = 1 - p,$$

$$\text{number of questions under each condition} = n,$$

then $2npq$ gives the variance of the difference between conditions for that subject. The variance of the total difference for all subjects is of course equal to the sum of the individual variances, and the total difference was tested against it; the 0.05 level requires a critical ratio of 2, and the 0.01 level one of 2.6.

The critical ratios for the comparisons mentioned in the results are given in Table 2. Wherever a ratio is accepted as significant with a value between 2.0 and 2.1 (that is, in border-line cases), analysis of variance was also carried out. For this analysis when the number of possible errors under each condition was eight or less, each score x was transformed to $\sin^{-1} \sqrt{x}$, the usual transformation for data of this type. The values of t from analysis of variance are given in brackets in Table 2 for these cases; they are more questionable than the methods already described, since the assumptions of analysis of variance are not valid here, but they provide some assurance that more familiar methods do not give wildly different results. One comparison in group K/S and one in group NR-K are the most doubtful and are considered in more detail in the section on results.

One comparison, that of buzz and between-buzz errors for group NR, was so marked that every subject in the group showed the effect. This result was therefore treated as significant from binomial theorem without any further test.

IV. RESULTS

Effects of an irrelevant stimulus. Group NR acts as a control for the other groups, since these subjects had to carry out the task without reacting to the buzzer. It is notable that even so they made more mistakes when the buzzer sounded; buzz errors were more frequent than between-buzz errors, $p < 0.001$. Part of this interference may be due to cochlear masking, as the signal-noise ratio is only unity.

Errors at the instant when another reaction is required. The major feature of these results is this: when an extra signal occurs during listening to speech, and this signal is not to be ignored but to produce some action from the listener, the speech is impaired both at the instant of this extra response and also between responses. Previous practice at the extra reaction helps to reduce the errors between but not during the signals for these extra reactions. The evidence for these statements is as follows.

The interference at the instant of response is shown by the fact that group K shows more buzz errors than group NR, $p < 0.05$. But this interference is not reduced by previous practice at key-pressing to the buzzer, by previous practice at selective listening, or by change in the reaction required from foot-stamping to key-pressing; groups KP, LP-K, and S-K all showed scores for buzz errors closely similar to those of group K (critical ratio < 1 in each case).

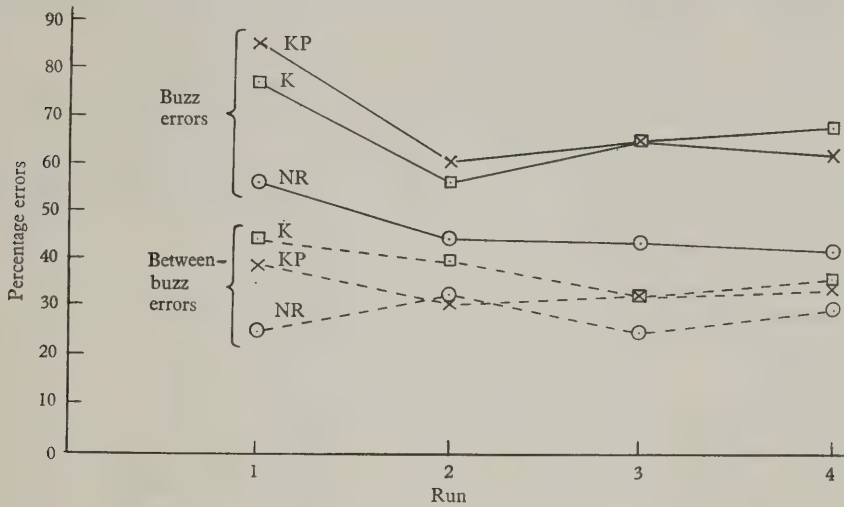


Fig. 2. The performance of the three most important groups.

Table 1. *Percentage of errors under each condition*

No. of subjects	Group	Conditions	Buzz errors				Between-buzz errors			
			Run 1	Run 2	Run 3	Run 4	Run 1	Run 2	Run 3	Run 4
12	NR	Ignore buzz	56	44	44	42	25	32	25	30
12	K	Press key to buzz	77	56	65	69	42	40	33	37
10	KP	Press key, previous practice at key-pressing	85	60	65	62	39	31	33	35
12	NR-K	Runs 1 and 2, ignore buzz. Runs 3 and 4, press key	63	35	83	69	40	35	34 +	39 +
12	S-K	Runs 1 and 2, stamp foot. Runs 3 and 4, press key	88	67	60	69	45	41	41	35
12	LP-K	Press key, previous practice at selective listening	79	63	54	71	45	50	43	43
10	K/S	Press key, or stamp foot if visual signal with buzz	92	78	77	80	38	38	29 +	27 +
12	K/SP	As K/S, previous practice at these reactions	85	71	71	73	33 +	33 +	30 +	26 +

For significance see text. Entries marked + exclude after-buzz errors and so are not directly comparable to the others given.

Group K/S was required to make a two-choice reaction when the buzzer sounded. In the first half of the session they showed more buzz errors than group K, the corresponding simple reaction group, $p < 0.05$. In the second half of the session, however, the subjects began to treat the situation as effectively one-choice, starting to press the key to every buzz and correcting the response later if incorrect (see data given in the section on controls below). Correspondingly the number of buzz errors ceased to be significantly greater than

in group K (C.R. = 1.53). A similar type of reaction was adopted even in the first half of the session by group K/SP, who had been practised previously; and equally their buzz errors were not significantly different from those of group KP (C.R. < 1).

Table 2. *Critical ratios*

Comparisons between different groups (tau tests)				Comparisons within same group (test on variance of proportions)			
Type of score	Groups compared		C.R.	Type of score compared		Group	C.R.
Buzz	K	NR	2.41	Buzz run 1	Buzz run 2	K	2.93
Buzz	K	KP	<1	Buzz run 1	Buzz run 2	KP	2.95
Buzz	K	LP-K	<1	Buzz run 1	Buzz run 2	LP-K	2.18
Buzz	K	S-K	<1	Buzz run 1	Buzz run 2	S-K	2.72
Buzz (first half of session)	K/S	K	2.21	Buzz run 1	Buzz run 2	NR	1.37
Buzz (second half of session)	K/S	K	1.53	Buzz run 1	Buzz run 2	NR-K	3.06
Buzz	K/SP	KP	<1	Buzz run 3	Buzz run 4	NR-K	2.05 (1.90, d.f. 11)
Buzz (second half of session)	NR	K	2.80	Before buzz	After buzz	K	<1
Buzz (second half of session)	NR	S-K	1.91	Before buzz	After buzz	LP-K	<1
Buzz (second half of session)	NR	LP-K	1.92	Before buzz	After buzz	S-K	<1
Buzz (second half of session)	NR	KP	1.79	Before buzz	After buzz	NR	<1
Buzz run 3	NR-K	S-K	2.12	Before buzz	After buzz	KP	<1
Between buzz (first half of session only)	NR	K	2.47	Before buzz (first half of session)	After buzz (first half of session)	K/S	<1
Between buzz (first half of session)	NR	S-K	2.07 (2.21, d.f. 22)	Before buzz	After buzz	K/SP	2.43
Between buzz (first half of session)	NR	LP-K	2.05 (2.35, d.f. 22)	Before buzz (second half of session)	After buzz (second half of session)	K/S	2.97
Between buzz (first half of session)	NR	KP	1.06	Before buzz (second half of session)	After buzz (second half of session)	NR-K	2.02 (1.75, d.f. 11)
Between buzz (first half of session)	KP	K	2.18	Key-pressing questions	Foot-stamping questions	K/S (first half)	<1
Between buzz (first half of session)	NR	K/S	2.12	Key-pressing questions	Foot-stamping questions	K/S (second half)	2.02 (1.65, d.f. 9)
Between buzz (first half of session)	NR	K/SP	<1	Key-pressing questions	Foot-stamping questions	K (second half)	<1
				Key-pressing questions	Foot-stamping questions	K/SP (first half)	3.16

Values in brackets are those for *t* tests on the border-line cases. They are for comparison only, as the assumptions of analysis of variance are not justified in this data.

Thus in all these cases where the subjects are making only the simple choice whether to react or not, buzz errors are the same; although when the subjects have also to choose between two reactions, more buzz errors are found.

Errors between the extra reactions. The interference during the time between reactions appears in between-buzz errors; the comparisons of major interest are for the first half of the session, and the values of *p* given are for this half only. Groups K, S-K and LP-K, who made simple reactions without previously practising them, gave more between-buzz

errors than group NR, $p < 0.05$ in each case. Group KP, who had practised key-pressing, gave no significant difference (c.r. = 1.06). This group was significantly better than group K, $p < 0.05$.

In the two complex reaction groups, the unpractised group K/S gave more between-buzz errors than group NR, $p < 0.05$. The practised group K/SP did not (c.r. < 1).

In all unpractised cases then, there is a rise in between-buzz errors caused by requiring a reaction to the buzz, this rise failing to appear after previous practice on the required response. It thus represents a type of interference distinct from that shown by buzz errors.

It remains to note that no group was significantly different from group NR in the second half of the session, though the c.r.'s were usually over 1, and the absence of significance may be due to the low sensitivity of the statistical techniques.

Controls necessary for the above conclusions. Further evidence on points of detail is needed to support the statements already made, and is presented in this section to avoid confusing the reader. The first point requiring more elucidation is the difference between before- and after-buzz errors. This is important because the interference shown by between-buzz errors might have been the spurious result of slow responses to the buzzer interfering with the questions heard immediately after the buzzer. But if this were so, after-buzz errors would be greater than before-buzz errors, and this is not true. For all simple reaction groups except group NR-K, which will be considered separately, the differences were very small and of variable sign, all c.r.'s < 1 . The between-buzz interference is not, therefore, due to slow reactions.

By a similar analysis we can detect the point at which the complex reaction groups, K/S and K/SP, begin to treat their task as a simple reaction. Group K/S gives before- and after-buzz errors equal, c.r. < 1 for the first half of the session. In the second half, however, the method of performance has clearly changed, since after-buzz errors are more frequent than before-buzz ones, $p < 0.01$. At the same time there is some evidence that key-pressing responses begin to give less buzz errors than foot-stamping ones, $p < 0.05$, by the proportion test though not by analysis of variance. The latter difference did not appear in the corresponding questions for group K nor in the first half of the session for group K/S. This harmonizes with the data already given and suggests that this group were, by the second half of the session, treating key-pressing as more likely and correcting the response if foot-stamping was required. The connexion between high after-buzz errors and preferential treatment for key-pressing is supported by group K/SP, which shows the same pattern in the first half of the session and more significantly. Key-pressing gave less errors than foot-stamping, $p < 0.01$, and after-buzz errors were more numerous than before-buzz errors, $p < 0.05$.

It is also desirable to know how far exposure to the situation without a response being required may produce the same effects as practice, and this is the function of group NR-K. In the second half of the session, when called upon to react to the buzzer it had previously ignored, this group showed an increase in buzz errors not only to the level of the simple reaction groups, but beyond it, and returned to that level only after another run. This improvement is just significant, $p < 0.05$.

A more satisfactory test is the significant difference in buzz errors on run 3 between this group and group S-K, who changed from one reaction to another. Group NR-K is also the only one of the simple reaction groups to show a marked excess of after-buzz errors over

before-buzz ones. The significance is border-line, reaching the 0.05 level by the proportion test but not by analysis of variance; but it may indicate an undue frequency of slow reactions or after-effects of response in this group. The complete absence of both effects in group S-K suggests that previous exposure to a stimulus without reaction may sometimes be more serious in its effects than practice of a different reaction. This has been predicted previously on the basis of other experiments (Broadbent, 1953). It should be noted that higher levels of practice on one response will presumably give the usual negative transfer effect on changing response.

Finally, we should note the effects of practice, not on one reaction by itself, but on the combined tasks. There is a decline in buzz errors after the first run in all the simple reaction groups, $p < 0.05$ in each case. (The complex reaction groups are complicated by the known change in their mode of performance.) But a similar though insignificant decline appears in group NR, which ignored the buzzer, and becomes significant, $p < 0.01$, in group NR-K. It may be due to compensation for masking effects. The important comparison, therefore, is between the buzz errors of group NR and group K for the second half of the session, and for this difference $p < 0.01$. For each of the other groups taken alone $p < 0.05$ by a one-tail test. Within the limits of this experiment, then, practice on the combined tasks does not make it as easy to do the two tasks as to do one.

V. DISCUSSION

(a) *Simultaneous distraction is not greatly helped by previous practice.* The main finding of these results is clearly the division of the interference between two tasks into two parts. One part arises when two stimuli are presented for response simultaneously. It increases when a reaction is required as opposed to no reaction, and is increased even more by a two-choice reaction (when the latter is performed as such and not as one-choice plus correction). On the other hand, it is more or less unaffected by experiences previous to combining the two tasks. We may point out the relevance of certain concepts of communication theory. In that theory the 'information' contained in a signal is determined not by the intrinsic nature of the signal, but by the probability of its occurrence; in many cases, by the number of alternative signals which might have occurred but did not. Similarly, in these experiments the central task is not impaired merely by the occurrence of an extra event, but by the number of possible situations which might have occurred and from which the actual one must be discriminated. Now any system will have a limit to the amount of information which it can transmit in a given time, this limit being known as the capacity of the channel. It seems fair to describe the interference shown by buzz errors as due to the limited capacity of the human perceptual system, though such a description is of course only justifiable because it says more than the simple statement 'the man is doing too much at once'. The simple statement obscures the vital fact that interference is produced not only by what the man is doing but what he might have to do. This appears, therefore, to be a case in which the language of communication theory shows real advantages.

Such a view also implies that further degrees of practice would be unlikely to show different results, a possibility which no actual experiment can ever rule out. If the perceptual system is of limited capacity, practice on one of two tasks can only be effective by reducing the amount of information analysed at the time of the performance of the

task (group K/SP). Some such reduction usually occurs with practice, so that a skill may become more and more 'closed' and independent of outside events (Poulton, 1950); but there is likely to be some limit beyond which this cannot be done. It is noteworthy that the time taken for certain tasks may similarly be reduced down to a certain point which again depends on the minimum quantity of information needing to be gained (Crossman, 1953). In the latter case exact quantification has been employed, but it is not at present possible to specify the informational load demanded by most practical tasks. But this analysis suggests that practice *will not usually be able to abolish completely an interference between two tasks when the stimuli for them arrive simultaneously*. Equally Conrad (1954) has shown that in a complex task signals are more likely to be missed completely by the subject if they occur close in time to a response to some other signal.

(b) *Distraction between signals is reduced by practice*. The other part of the interference, however, is obviously reduced by practice. This is an interference with one task arising between the occasions when stimuli from the other task appear. It is apparent that the reduction of this interference will allow two practised tasks to be carried on at the same time, provided critical signals do not arrive simultaneously. This is the equivalent of the classical 'rapid alternation of attention', and appears to explain adequately why those who use multiple channel systems in practical situations place such emphasis on experience.

When a task is 'automatized' by practice, we may therefore say that two kinds of change are taking place. Although the task still appears the same, the person carrying it out now requires less sensory information for successful performance, and also the task ceases to make any demand on him during intervals in its execution. These changes bear a close resemblance to the two features of automatization which were noted in the Introduction, though the change in phrasing has some advantages. We have already noted the parallel between 'interference of two activities' and 'limited capacity of the perceptual system'; and have also noted the reasons for preferring the latter language. The parallel between reduced introspective report and the ability to deal with stimuli alternating from two tasks is perhaps less clear. Yet an interference with one task between successive stimuli from another task must mean that some activity connected with those stimuli is persisting between them, that they are in a sense related to one another and to other intervening events. Introspective accounts of reaction would equally appear to demand relation of the particular stimulus to the whole sequence arriving at the individual. Since both functions also cease to appear with repeated practice, it is tempting to postulate that they are identical.

It is interesting to note that a few other experiments using rather different types of situation support these general conclusions; data drawn from this task alone are naturally a dangerous ground for generalization. Gregg & Brogden (1952) showed that thresholds in one sense obtained with simultaneous stimulation through another were higher if both stimuli were to be reported, and actually lower if only one was to be reported. This corresponds to the first type of interference due to informational load. Another related experiment is that of Baker, Wylie & Gagné (1951) on the effects of interference in motor skill, using various degrees of practice on the task which was to be scored. They confirm that interference continues to exist up to the highest level of practice used, which we have already stated as a consequence of regarding part of the interference between two tasks as due to informational load. Bahrick, Noble & Fitts (1954) showed that interference between

two tasks is reduced with practice when the stimuli for one of them arrive in a predictable sequence, but not when the stimuli arrive in an irregular sequence and thus continue to convey information.

Interference with one task when stimuli for another were not present was shown by Mackworth (1950); his subjects did worse on the Clock Test when told that a telephone might ring. But practice effects were not studied in this case. Baker *et al.* (1951) showed that when a new task was added to a motor skill at various levels of practice, there was always an extra temporary interference which died away until all groups showed equal amounts of interference. This temporary decrement may correspond to the between-buzz interference; but in general experiments on combining two tasks have not distinguished between physical simultaneity of the stimuli and alternation of stimuli from the two tasks. There is therefore much room for further experiment on the view that an 'automatized' task is being carried out on the minimum amount of information, and that it interferes with other tasks only at the instant when that information is presented. But existing evidence favours such a view.

This work owes much to Sir Frederic Bartlett, F.R.S., Dr N. H. Mackworth and Dr E. C. Poulton, for general encouragement and advice, to Miss V. R. Cane on the statistical side, and to the Royal Navy for supplying subjects and equipment.

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MIND WANDERING

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The experiments here briefly described were primarily an attempt to devise and evaluate methods for investigating certain basic characteristics of attention. The two questions we asked may be put as follows: (i) How often does the mind wander? (ii) Does the frequency or duration of the mind wandering vary in any systematic way? The experiments were intended as preparatory to a search for the factors determining the presumed variations.

Three methods were employed:

(a) Each member of a class listening to a lecture, or of a group listening to gramophone music, was provided with a bell-push and instructed to press it whenever he became aware that his attention had returned to the lecturer or the music. The bell-pushes were so arranged that when any one was pressed, a corresponding torch-bulb lit up in an adjoining room and was recorded either on a continuously moving film or by observers. By this means up to fifty subjects could be studied at the same time.

(b) Observers watched a class of normal children through a one-way screen. They noted when a child seemed not to be attending to the teacher, the criteria of inattention being, for example, looking out of the window, talking to another child, fidgeting, falling asleep and so on.

(c) Observers watched a small class of deaf children through a one-way screen. Attention to the teacher required lip-reading on the part of the child, and the observers noted when each child was not looking at the teacher.

Methods (b) and (c) were indirect and enabled us to estimate how long each child's attention wandered on any occasion. Method (a) was direct, though it yielded only the frequency of wandering.

We found that in the lecture classes the subjects, on the average, pressed the bell-push about four times during a 40 min. period. The frequency was a little higher in the music group. In both cases individual differences were relatively enormous. Of the 165* subjects in the lecture classes, 14% did not press the bell-push at all, about 9% pressed it once, and four subjects pressed it twenty times or more. The mode was about three or four in the different lectures. Much the same results were obtained with the group listening to music.

Table 1. *Mean frequency with which bell-push was pressed during lectures, in 5 min. intervals from commencement (N = 165)*

Minutes ...	5	10	15	20	25	30	35	40
Mean	0.27	0.52	0.56	0.61	0.62	0.64	0.55	0.43
S.D.	0.62	0.81	0.86	0.94	0.88	1.11	1.07	0.83

As may be seen from Table 1 the mean frequency with which the bell-push was pressed during the lectures, in intervals of 5 min., ranged from 0.27 to 0.64, the corresponding

* Built up in four sessions with a single group of about forty.

standard deviations ranging from 0.62 to 1.11. In the case of the music groups the mean frequency of pressing ranged from 0.18 to 0.87, with standard deviations ranging from 0.46 to 1.47 (see Table 2). Apart from the infrequency of pressing during the initial 5 min., no significant variation in frequency of pressing with time was observed.

Table 2. *Mean frequency with which bell-push was pressed by members of music group, in 5 min. intervals from commencement (N=33)*

Minutes ...	5	10	15	20	25	30	35	40	45
Mean	0.18	0.67	0.76	0.82	0.58	0.82	0.87	0.65	0.80
S.D.	0.46	0.94	1.02	1.47	0.82	1.19	0.90	0.87	1.07

Subjects reports revealed the considerable difficulties many of them had in carrying out the instructions. Pressing the bell-push was found to be distracting and itself to divert the attention. It was often hard to decide when the attention seemed to have returned. Any particular wandering might feel like a definite lapse or daydream or, at the other extreme, like a slight oscillation or mere flicker of attention. When the mind wanders it may be to ponder more deeply on the lecturer's theme without being a lapse in any sense or it may be due to a 'pull' from some other direction which has nothing at all to do with the topic of the lecture.

In the experiments with twenty-nine normal children, judging by the criteria we employed, a child's attention seemed to return, on the average, about once a minute. The means for 5 min. intervals ranged from 4.95 to 5.73, with standard deviations ranging from 2.45 to 3.33. The duration of the wanderings on this basis ranged from a momentary flicker too short to be timed to about 3.75 min., with a mean of 13.74 sec. The details are set out in Table 3. Rather similar results were obtained from the observations of the deaf children by method (c), see Table 4. The mean duration of their inattention was about 4.8 sec.

Table 3. *Mean frequency with which the attention of normal children seemed to return, in 5 min. intervals (N=29)*

Minutes ...	5	10	15	20	25	30
Mean	5.42	5.21	5.68	4.95	5.38	5.73
S.D.	2.92	2.67	3.33	3.47	2.45	3.13

Table 4. *Mean frequency with which the attention of deaf children seemed to return, in 5 min. intervals (N=4)*

Minutes ...	0-5	5-10	10-15	15-20	20-25	25-30	30-35
Mean	4.75	6.25	5.25	7.25	4.0	5.0	4.5
σ	1.61	2.15	6.93	4.03	2.35	1.87	2.68

Further use of direct and indirect methods on the same subjects, with the experimental material and conditions held constant, may enable us to validate these methods. If the two types of methods yield similar results, then the simpler and more expeditious direct method of bell-pushing might usefully be employed in investigating the two questions formulated above and allied problems.

We are indebted to Prof. Ewing for providing facilities to observe the deaf children.

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THE INFLUENCE OF MENTAL SET ON PROBLEM-SOLVING

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In his laboratory manual, Foster (1923) includes an experiment which aims to demonstrate the influence of mental set on speed of problem-solving. He uses six different lists of five-letter anagrams. Two series, containing miscellaneous words, are presented with an indefinite set, the subject being told nothing about the general nature of the words involved. Two series comprise words which 'have something to do with eating' and another two series contain words 'representing things about the house'. Before attempting these, the subject is informed of the classification. Foster reports only that 'typical results show that the median time with indefinite set is about 2.5 times as long as for definite set' (1923, p. 152) and that 'a typical median time for transposing anagrams with indefinite set is 25 sec.; for transposing anagrams referring to eating, 10 sec.; for anagrams referring to house, 10 sec.' (Foster & Tinker, 1929, p. 6).

It seems likely that these results demonstrate the facilitating effect of definite as opposed to indefinite set. Nevertheless, an unambiguous interpretation is difficult to make. There is no adequate control of practice and fatigue effects. Also the specified and unspecified series employ different anagrams and there is no guarantee that the difference between the times taken to solve these is not attributable to a mere difference in their intrinsic difficulty. This note reports a class experiment in which Foster's materials and design were modified. The result demonstrates, of course, only one of the ways in which set can be established, and only one of the ways in which its effects can be observed (other investigations of set have been reviewed recently by Johnson, 1955). However, the experiment confirms, under better-controlled conditions, Foster's original findings. Moreover, it provides an interesting and instructive experiment for use in the teaching of experimental psychology.

The experiment was carried out with four laboratory sections of a first-year class in experimental psychology. A total of seventy-two students (thirty-five men, thirty-seven women) took part. The students were divided into groups of three, each member of the group acting, in turn, as subject, experimenter, and recorder. The material consisted of fifty-four five-letter anagrams, each of which permitted of only one solution. The solutions were nouns which fell into nine different categories as follows:

1. Animals: sheep, horse, tiger, skunk, camel, zebra.
2. Trees: birch, rowan, aspen, beech, hazel, holly.
3. Musical instruments: piano, flute, cello, organ, banjo, viola.
4. Birds: stork, eagle, goose, robin, heron, raven.
5. Fruit: peach, grape, olive, lemon (melon), prune, apple.
6. Parts of the body: wrist, mouth, waist, ankle, thigh, chest.
7. Countries: India, Spain, Wales, Italy, Japan, Tibet.
8. Wearing apparel: glove, skirt, apron, scarf, dress, shirt.
9. Flowers: pansy, lilac, daisy, tulip, lupin, poppy.

Each subject attempted two lists of six anagrams, one specified and the other unspecified. The specified lists used, for example, by the first group were lists 1, 2, and 3 above, while

each unspecified list contained six nouns, one selected from each of the remaining categories. Using twelve groups of three subjects, lists were prepared so that: over the thirty-six subjects, each noun occurred the same number of times, i.e. twice, as specified and as unspecified; practice and fatigue effects were counterbalanced, half of the subjects attempting a specified list followed by an unspecified, and half attempting first an unspecified and then a specified list; in no group did an experimenter or recorder experience a noun which he would encounter while acting as subject.

Each group was given a six-page booklet, on each page of which the appropriate list was typed in capital letters on heavy white paper. Each list was headed either 'Unspecified' or given a specific title, e.g., 'Animals'. Over the first list, the experimenter placed a long strip of hard-board in the centre of which was an aperture just large enough to expose a single anagram. The experimenter then moved the aperture down to expose the heading and told the subject whether the list was unspecified or specified in a particular way. He then exposed the first anagram for 30 sec. If the solution was given in this time, the experimenter waited until the recorder had noted the time taken for solution and then exposed the second anagram. If the anagram was not successfully transposed in 30 sec., a failure was recorded and the next anagram exposed. This procedure was continued until the end of the list. The same subject then attempted the second list. Members of the group then exchanged functions and the next two lists were presented to a new subject. This done, the third subject attempted the last two lists. Throughout, the recorder was responsible for timing the experiment by stop-watch and for entering the results in a specially prepared table. An hour gave the students time to perform the experiment, calculate the results for their own group of three, and discuss both their findings and the design of the experiment.

The results from the seventy-two subjects were as follows:

Median time to solve anagrams in specified lists	= 3 sec.
Median time to solve anagrams in unspecified lists	= 12.5 sec.
Total number anagrams failed in specified lists	= 35 (8.10 %)
Total number anagrams failed in unspecified lists	= 129 (29.86 %)

These results show clearly that the more specific the set the easier is the problem to solve. It is to be noted that the set involved here may not derive entirely from the instructions given but may, in part, have been established in the course of transposing a series of homogeneous anagrams. Even without specific instructions, a subject might, after solving four or five 'animal' anagrams, be set for an animal name in the sixth anagram. Such a task-induced set can easily be demonstrated in the classroom by means of the water-jar problems of Luchins (1946). However, the design of the present experiment does not permit any conclusion regarding the extent to which set established in this way contributes, if at all, to the results obtained.

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(Manuscript received 11 June 1955)

CRITICAL NOTICE

By W. RONALD D. FAIRBAIRN

The Technique of Psycho-Analysis. By EDWARD GLOVER. London: Baillière, Tindall and Cox. 1955. Pp. x+404. 35s.

This volume, like Caesar's Gaul, is divided into three parts. Part One consists in an expanded version of the book published by the author in 1928 under the same title as the present volume and comprising six lectures delivered as a training course at the London Clinic of Psycho-Analysis in the days before systematic instruction in psycho-analytical technique had been initiated. The original book was published at a time when Freud's ego-psychology had only comparatively recently been formulated and his concepts of the super-ego and the id were still novel; and, in view of the considerable developments which psycho-analytical theory has undergone since that date, the author has had to consider how far the general outline of the original book could be preserved. In deciding to preserve it, he has been influenced by the consideration that, whatever complications may have been introduced into the technique of interpretation by theoretical developments, the 'analytical situation' as such and the phenomena of 'transference' continue to be governed by a few simple laws. He has also been influenced by the consideration that the practical difficulties experienced by students of psycho-analysis remain more or less constant. Part Two comprises an analysis of the results of a questionnaire concerning the details of psycho-analytical technique completed by twenty-four practising British psycho-analysts in 1938. The results of this research were originally published in 1940 under the title, *An Investigation of the Technique of Psycho-Analysis*; and they may be regarded as providing a representative picture of the practice of members of the British Psycho-Analytical Society at the date of the Questionnaire. Part Three contains three papers of varying dates, each of which has already appeared in *The International Journal of Psycho-Analysis*; and each represents a contribution towards an attempt to determine what is and what is not to be regarded as psycho-analytical treatment in the strict sense.

The book as it stands represents the most systematic and comprehensive study of the technique of psycho-analysis known to the reviewer; and, if the disquisition in Part One may appear to the reader somewhat *ex cathedra*, it must be remembered that this part originated as a lecture course for students of psycho-analysis seeking authoritative guidance in a field beset with difficulties. The reader can hardly fail to be impressed by the complexity of the problems which confront the psycho-analyst, and by the high standard of knowledge, training, judgement and skill required of him. However, Dr Glover takes pains to discourage any tendency to regard psycho-analysis as an esoteric cult, and to disillusion such as may be inclined to accept the myth of the psycho-analytical superman. As he makes clear, the psycho-analyst is merely an ordinary human being who, through his own analysis, has acquired an unusual degree of insight into the nature of unconscious mental processes and has achieved an appreciable mitigation of his own inner conflicts. In his view, any extravagant claims for the superiority of the analysed person were administered a *coup de grâce*, once and for all, by Freud's discovery of the id as an integral part of human nature; and he deprecates any inclination on the part of the analyst to identify himself with 'the all-powerful parent who knows everything and does no wrong'. He likewise makes it plain that, whilst the analyst must necessarily be dispassionate in his approach to his patient's problems and avoid the pitfalls of the counter-transference, he must never fail to maintain a human approach. Considerable space is naturally devoted to problems of interpretation, this being the main therapeutic instrument of the practising analyst; and emphasis is laid on the general principle that 'interpretation of unconscious phantasy must as a rule be timed to take effect when the defensive systems have been really weakened'.

The problem considered in Part Three, viz. that of differentiating psycho-analytical therapy proper from other forms of psychotherapy, obviously makes a special appeal to Dr Glover. Among the criteria of psycho-analysis to which he attaches special importance are (1) integrity and non-tendentiousness of interpretation, and (2) the maintenance of an attitude of detachment or neutrality on the part of the analyst. He also considers it as distinctive of psycho-analytical treatment that it aims not so much at strengthening ego-defences as at penetrating them with a view to unmasking unconscious processes which distort 'normal ego-defences', and that, in pursuing this aim, it abjures both suggestion and any unchecked exploitation of rapport. He further claims that any interpretative technique which is not based upon

unreserved recognition of such fundamental Freudian concepts as repression and infantile sexuality culminating in the Oedipus situation forfeits all claim to be called psycho-analysis, and that the same applies to any form of psychotherapy which dispenses with analysis of the transference.

According to Dr Glover any therapeutic effects produced by 'inexact interpretation' must be regarded as dependent upon suggestion; and, indeed, he appears to regard suggestion as a form of therapy based upon an exploitation of inexact interpretation. It may well be asked, however, how far the effects even of 'exact' interpretation can be regarded as wholly independent of the authority and the general attitude of the analyst; and such a query is all the more justified in view of the fact, revealed by the results of the Questionnaire, that, whilst 'a number of orientated and practising analysts holding to the fundamental principles of psycho-analysis varied in their methods in every imaginable way', the therapeutic results obtained appeared to be 'much the same' (p. 373). This remarkable finding is perhaps less remarkable than a conclusion registered by Dr Glover regarding the perplexing problem of the length of psycho-analytical treatment, viz. 'that the earlier analysts were accustomed to conduct analysis of six to twelve months' duration which as far as I can find out did not differ greatly in ultimate result from the results claimed at the present day by analysts who spin out their analyses to four or five years' (p. 383). This conclusion may well give rise to some heart-searchings. The obvious doubt which it raises in the critical mind is whether what the earlier analysts did and what contemporary analysts do are really the same thing. This is a question which it is difficult to answer on the basis of available data, owing to what Dr Glover describes as 'the curtain of uncommunicativeness behind which psycho-analysts are only too prone to conceal their technical anxieties, inferiorities and guilts' (p. vii); and it should be noted that Freud himself provided comparatively little information regarding the technical details of his handling of patients. However, it is difficult to see how this question can be satisfactorily answered until more light is thrown upon the more basic question how psycho-analytical treatment produces its effects. The view current in the earlier days of psycho-analysis was that the therapeutic effects of analysis were produced by the dissolution of more or less localized 'complexes' combined with abreaction of the affects bound in these complexes; but this was in the days before Freud's ego-psychology had been formulated. The view most widely held in contemporary psycho-analytical circles and endorsed by Dr Glover himself is that the therapeutic effects of analysis depend upon (1) modifications of ego-defences such as to enable the ego to develop more satisfactory methods of dealing with id impulses (the id itself being, of course, by hypothesis incapable of modification), and (2) modifications of the super-ego such as to render it less primitive and less exacting in its demands upon the ego. However, among those influenced by Kleinian thought there is a tendency to attach therapeutic importance to the introjection of the analyst as a good object. The importance of this factor is regarded by Dr Glover as having been exaggerated. He is, of course, well known to be unsympathetic towards the Kleinian school; but he himself describes 'the humane relation of the transference' (p. 370) as a factor in the therapeutic process, and makes himself responsible for the suggestion that 'in the deeper pathological states a prerequisite of the efficiency of interpretation is the *attitude*, the true unconscious attitude of the analyst to his patients' (p. 372). All this suggests that the analyst does not merely fulfil the dual functions of a screen upon which the patient projects his phantasies and a colourless instrument of interpretative technique, but that his personality and his motives make a significant contribution to the therapeutic process. Dr Glover himself says in one place, 'It is obvious that many people cure themselves through their unconscious human contacts' (p. 372); and the question therefore arises whether the actual relationship between the patient and the analyst in the analytical situation is not the decisive therapeutic factor. If this be so, it may partly explain not only why analysts using varying methods obtain similar results, but also why the results obtained nowadays in analyses lasting four or five years are no better than those obtained in analyses of six to twelve months by the earlier analysts. For it may well be that the attitude of the contemporary analyst, steeped as he is in ego-psychology and impressed as he cannot fail to be by the inherent difficulties of effecting a lasting alteration in mental structure, makes a less therapeutic impact upon the patient than the more naïf attitude of the earlier analyst who regarded his task as comparatively simple and really believed that psycho-analysis provided an answer to all human ills. At any rate, it would appear an urgent demand that Dr Glover's masterly study of the technique of psycho-analysis should be supplemented by some more profound research than has hitherto appeared into the fundamental problem how psycho-analysis really works.

PUBLICATIONS RECENTLY RECEIVED

An Introduction to Psychology. By HARRY W. KARN and JOSEPH WEITZ. New York: John Wiley and Sons; London: Chapman and Hall. 1955. Pp. xi+315. 32s.

Both for what it achieves, and for what it attempts to do, this book merits attention. Its aim is to interest, and impart accurate information to, the student undertaking only a *brief* course in psychology. Precisely because it is simpler and considerably less technical than the usual first-year text-book, this volume may meet the need for a book suitable as preliminary reading for the intending student of psychology.

This well-written book contains many illustrations, including line diagrams of apparatus, and makes frequent reference to specific experiments. The two introductory chapters place emphasis upon scientific method, and succeeding chapters deal with such subjects as perception; remembering and thinking; frustration and emotion; personality; and instruments of measurement. On occasion greater simplicity has been achieved by the authors' decision 'to take one systematic position and stick to it'; thus learning is treated from a reinforcement theory standpoint. Certain criticisms might be made. For example, the account of physique and temperament pays scant attention to the findings of actual investigations in this area. Again, the functional psychoses are discussed in an over-generalized and unconvincing way; moreover, it is difficult to see how a book which does not mention hallucination can deal adequately with a topic like 'schizophrenia'. Somewhat unusual is the book's avoidance of any reference to Freud and psycho-analysis, and, although perception is dealt with, there is no mention of *Gestalten* or the Gestalt school.

Although this book contains little that will be new information for the psychologist himself, he may well wish to recommend it, not only as 'summer reading' for intending students, but also to his friends and non-psychologist colleagues. It is an informative, interesting, and lucidly written introduction to the subject.

PETER MCKELLAR

Scientific Method in Psychology. By C. W. BROWN and E. E. GHISELLI. New York and London: McGraw-Hill Book Company, Inc. 1955. Pp. ix+368. 45s.

This new book by Brown and Ghiselli is simple, straightforward, clear, and written at the level of the typical American undergraduate student. It sets out precisely and without any sesquipedalian elaboration modern notions of scientific method, interspersing the discussion with many relevant and well-chosen examples. At its level the book does an excellent job and may be thoroughly recommended. It would be invaluable, particularly for arts students with little knowledge of the scientific method who have taken up psychology for the first time and need a gentle but firm introduction to the scientific way of thinking.

On the debit side are certain features which follow inevitably from the plan and purpose of the book and from the level at which it is written. Views are stated rather than debated, even when alternative views are possible. Certain important concepts do not appear at all, although they play an important part in modern thinking. (We may instance 'operational definition' and 'intervening variable'.) Some of the chapters dealing with psychophysical methods and elementary statistics might, with advantage, have been sacrificed for a fuller discussion of some of these points. These chapters are inevitably too short to be sufficient without further reading, and would normally be treated in a separate course anyway.

On the whole, however, the good points of the book very much outweigh those criticized, and *Scientific Method in Psychology* fills an empty place in the teaching of psychology in a more than adequate manner.

H. J. EYSENCK

Clinical versus Statistical Prediction. By PAUL E. MEEHL. Minnesota University Press; London: Cumberlege. 1955. Pp. x+149. 24s. net.

Much of the research designed to illustrate the relative success of clinical *vis-à-vis* statistical prediction has been vitiated by the lack of clarity with which these seemingly unambiguous terms have been employed. It is interesting that Dr Meehl succeeds in finding only twenty studies in which the two methods are sufficiently well differentiated for meaningful comparisons to be made.

The theoretical analysis of the problem is carried out with the author's customary care and rigour. After reminding the reader of the difference between the validating and the structural or analytic use of statistics, he suggests that a frequent source of confusion in past work has been in the failure to distinguish between the way in which data have been obtained, and the method by which they are combined to arrive at the required prediction. Psychometric data—the results of properly standardized tests—combined mechanically, i.e. by an application of an equation or table to the data, afford a prediction which has unquestionably been derived by a 'statistical' process. When non-psychometric data are combined non-mechanically or informally—as, for example, when an employer, after studying work histories and the foreman's report, conducts an interview to decide the most suitable candidate for promotion—such a procedure is clearly 'clinical'. Between these two clear-cut examples we have the less easily classifiable cases where psychometric data are combined non-mechanically, non-psychometric data are combined mechanically, and finally the example typified by most case-conferences where the data, psychometric and otherwise, are combined informally to yield a diagnosis and prognosis.

Dr Meehl continues his analysis by considering the view of Lundberg, Sarbin and others that the processes by which the clinician moves from his data to a prediction are logically indistinguishable from the mechanical application of an actuarial table or regression equation to the same data. What differences there are are quantitative rather than qualitative, and favour the overtly statistical method, since its operations are explicit and its units precise and well defined. If the clinician arrives at his conclusions by a process which is essentially if implicitly actuarial, he lays himself open to the charge that he is functioning as 'a second-rate substitute for a Hollerith machine'. One feels that Meehl finds this charge proven, but provides, for the philosophically sophisticated at least, a possible plea for leniency in the non-frequentist logic of Carnap. The practical implications of this point of view lead one to expect that when the two methods are compared the statistical should be found to be more reliable and more efficient. The evidence reviewed here adequately confirms this expected superiority of the actuarial method.

The scope of the studies cited is however rather limited, dealing largely with the prediction of scholastic success or failure, recidivism, and the recovery from psychotic illness. This is not a very adequate cross-section of the clinician's predictive activity, and of course the clinician as therapist is outside the scope of the discussion. Our doubts about the conclusiveness of the evidence examined are strengthened when we consider that in these studies the actuarial methods used are refined, reliable processes of known value, whereas we have no information about the quality of the clinicians with whom the comparisons are being made. These limitations, fully recognized by Dr Meehl, are inherent in the available research material, and should not be considered an inadequacy in the book. Indeed, despite the admittedly sketchy empirical evidence, one feels that the burden of self-justification has been transferred to the clinician. There remains, however, a real need for carefully planned research in this area; the subtlety and flexibility of the clinical approach is perhaps unduly constrained by the need to predict recidivism and psychotic recovery. A more congenial research project may provide the clinician with a better opportunity of demonstrating the effectiveness of his approach.

The clinician and the statistician, however fiercely they may debate this issue, do so with the assurance that whatever the outcome they will be only slightly affected by it professionally; this happy position is not shared by the clinical psychologist. His professional status and success are directly dependent on the effectiveness with which he can make predictions. Unfortunately the psychometric tools and actuarial tables which would permit him to adopt a purely statistical approach have not yet been designed; while his psychiatric colleagues rightly expect more of him than the clinical experience and insights on which they rely. He is thus obliged either to restrict the range of his activities—not always a possible solution—or to effect some compromise in his methods. In practice he may have to supplement a relatively narrow range of diagnostic instruments, with the more or less inspired guesswork which passes for clinical intuition. To the scientifically trained psychologist this is a most uncomfortable state of affairs. However significant his contribution to the case-conference, he will remain rather a man without a country, until the controversy which Dr Meehl so brilliantly presents has been satisfactorily resolved.

J. DREWERY

The Child's Conception of Reality. By JEAN PIAGET, translated by MARGARET COOK.
London: Routledge and Kegan Paul. 1955. Pp. xiii + 389. 25s.

The psychology of childhood is beset with pitfalls, and overwhelmed with 'material'. The manual workers, industrious ants, have indeed amassed a colossal heap of facts—physiological, behavioural, psychometric, normative—and the clinicians a deal of artful rule of thumb, but little of it making any effective contribu-

tion to the development of a properly genetic psychology. This must, nevertheless, remain one of the foundation stones of a sound general psychology—essential, for example, to a proper appraisal of Michotte's work on causality, or of the Gestalt account of perception. That is why Piaget's latest adventures over the ill-charted sea of childhood are so important, and that not only, or indeed primarily, to the students of child development as such, but rather to the general psychologist.

The present book is a companion volume to the earlier *Origin of Intelligence in the Child*, and should be read in conjunction with it. In both the data are drawn from Piaget's carefully recorded observations—after the manner of Stern and Valentine—of his own three children during the first two years of life. These data are now analysed and re-analysed in order to demonstrate the pattern of development of (a) the object concept, (b) the spatial field, (c) causality and (d) the temporal field. The whole is summarized and brought together in a final chapter entitled 'The elaboration of the universe'. In the earlier work Piaget has analysed the origins of intelligence in order to elucidate how the forms of intellectual activity are constructed on the sensori-motor level. He now sets out to show how the real categories of sensori-motor intelligence are organized—'how the world is constructed by means of this instrument (of intelligence)'. Consequently the works stand as a vindication, as it were, of intelligence at the earliest stages, a firm assertion of 'the primacy of intellectual activity over ready-made structures, a primacy which Gestalt psychology seems . . . to neglect'. Piaget, therefore, is concerned first to enable us to understand how the child comes to the apprehension of external objects considered separately as items relatively detached, and then to demonstrate how by the same six stages (cf. *The Origin of Intelligence*) they come to be interconnected as parts of one world. The aim is to trace the movement from the vague and rudimentary apprehension of 'thing' or of space, tied to or implicit in behaviour that is initially reflex in character, to the acquired unity and differentiation of the external world and of the self as part of the world.

No psychological enterprise is more worth while; none is more difficult, and we may not complain if the discussion is difficult also. There is, indeed, an element of almost Chestertonian paradox in the simplicity of the observations—'games with children'—and the abstract level at which the argument is sustained. As Valentine has pointed out, the most reliable evidence as to what is genuinely innate in human nature, and what in the taken-for-granted bedrock of our experience is to be regarded as second nature, must be found in the study of human infancy. But this involves us in the closest scrutiny of the evidence that is produced, and warrants the most cautious acceptance of the interpretations that are based upon it. Piaget has never been afraid of bold and imaginative theorizing, and the new Piaget is no less courageous, no less imaginative, though less facile than the old. Yet there are certainly occasions where we may be permitted to doubt the inevitability of the interpretations he himself favours. For example, in Obs. 51 (p. 58), which turns upon a child's understanding (or misunderstanding) of adult language, the elucidation appears to be precarious. It is perhaps doubtful also whether the factual material presented is generally sufficient to bear the superstructure that has been erected upon it. Nevertheless, the evidence upon which we shall rely must be of the kind that Piaget has collected, and further testing and observation must follow the lines of his prolific suggestion.

No brief review can do justice to this important book. Along with the earlier volume, it constitutes the most important contribution to the genetic approach in psychology that we have had from any writer in Europe or America for a quarter of a century. It is perhaps a pity that the difficulty that most readers will certainly experience should be increased by some oddities of the translator's vocabulary and the appalling length of some of the trailing sentences.

GEORGE SETH

Theories of Perception and the Concept of Structure. By F. H. ALLPORT. London: Chapman and Hall, 1955. Pp. xxii+709. 64s.

This book is written for psychologists who are interested in theory. As the title indicates, it has the dual purpose of surveying contemporary theories of perception and of launching a General Theory of Event Structure. This theory, introduced in the final chapter, is still in such a tentative and almost purely logical state of development that the author promises a more extended treatment of it in a forthcoming book. The theory starts from the fact that, in science, quantitative laws are not enough. It is also necessary to know something about the non-quantitative 'general format' or 'structure' of phenomena. This being so, it may be possible to derive 'structural' laws which go beyond mere qualitative description and are logically prior to quantitative relations. The author claims that such laws are conceivable and attempts to give some inkling of what they may be like. Because the theory is so undeveloped, it is difficult to evaluate. If the author's claims are justified, it will revolutionize the thinking not only of psychologists but of scientists

in general and also, presumably, of historians. But, as presented in this book, the concept of structure is not too clearly delineated; the reviewer feels that, so far, the theory says nothing which has not been said before in simpler terms; he is not even convinced that the problem of 'structure' is, in any sense, a real one. A final assessment of Allport's theory must await the appearance of his second volume.

The major portion of the book is devoted to a review of thirteen theories of perception. These theories are: Titchener's core-context; Gestalt theory; Lewin's field-theory; Hebb's cell-assembly; Werner's sensori-tonic field; Freeman's set; Helson's adaptation-level; Brunswick's probabilistic functionalism; Ames's transactional functionalism; Bruner and Postman's directive-state; hypothesis-theory; Hull's S-R theory; and cybernetic theory. As a survey, this part of the book—long enough to be a book in itself—suffers by comparison with, say, Hilgard's *Theories of Learning*. It disappoints on three counts. First, many of the theories are not theories at all, in so far as they aspire merely to offer generalizations about some delimited aspect of perception. This would not be important but for the book's tendency to criticize each theory for its lack of comprehensiveness and so, in some cases, forgo a more detailed critique. Secondly, the treatment is insufficiently factual. Only with the discussion of the more recent 'new look' work (in chapter 15) is it concerned with relatively concrete perceptual matters—and this is the most stimulating and perhaps the most constructive part of the whole book. Thirdly, the survey is incomplete. For example, Michotte's name is nowhere mentioned, nor is that of Piaget, who employs many concepts of the sort favoured by the author. The crux of the matter seems to be that Allport does not intend to follow the Hilgard pattern. He surveys primarily to emphasize the need for 'structural' laws. Gestalten, cognitive maps, 'hypotheses', brain-fields—he seeks to show that these concepts, and many more, represent unsuccessful attempts to deal with the problem of 'structure' in the field of psychology. Yet, to the reviewer, this is not the moral pointed by the survey. To him, the moral, if any, is that psychologists should not expect to find a 'general theory' of perception with greater confidence than physiologists would, say, a 'general theory' of digestion.

Allport attempts to do something really important, and the time is ripe for a work such as this might have been. It is all the more regrettable, therefore, that the book's two aims should defeat each other. A clear statement of Event Structure Theory is crowded out by the review of other theories which, for their part, are inadequately treated because they must serve to orient the reader towards the final theory. Nor is the reader assisted by the style of presentation. Helpful diagrams (such as appeared in the much more satisfactory statement of the theory in the *Psychological Review*) are eschewed. The writing itself is diffuse; since its contents close the book to the legendary 'interested layman', the psychologist reader would have preferred a less repetitious and more cogent version.

Despite the above criticisms, the book contains many good things and strongly recommends itself to all psychologists who experiment on and theorize about the phenomena of perceiving, learning and thinking. In it are raised again those basic problems, both philosophical and scientific, which it is so often tempting to ignore. In it are provocative verdicts on viewpoints cherished by many. In it, too, the advanced student will find useful starting points at which to begin his study of modern contributions to the psychology of perception. And it cannot fail to give the perception specialist a broader perspective. Although the book may not fulfil the promise of its early pages, it represents, nonetheless, a valiant, sincere and skilful attack on some of the most perplexing problems in psychology and whets the appetite for its successor.

I. M. L. HUNTER

The Actual-Genetic Model of Perception-Personality. By ULF KRAGH. Lund: C. W. K. Gleerup; Kobenhavn: Munksgaard. 1955. Pp. 394. Kr. 40.

The term 'actual-genetic' is translated from the German 'Aktualgenese', invented by F. Sander to denote the successive perceptions of a stimulus object which is at first only imperfectly seen. In fact, of all the stages preceding full perception, only those are included in which there is some identification of form and attribution of meaning. It was further suggested by Sander that these stages bore some definite and consistent relationship to the final percept; and also to the structure of the personality. The author of this book has put forward the additional hypothesis that the 'actual-genetic' series of partial perceptions may reflect the actual stages of development of the personality in relation to the principal events of emotional life.

In investigating this thesis, the author has used two types of perceptual experiment: (1) measuring the changes which occur in the size of the after-image when the latter is repeatedly projected on to a distant screen; (2) recording the changes which occur in tachistoscopic perceptions of drawings, photographs of

faces and T.A.T. pictures, with successive exposures of gradually increasing duration. It is difficult to understand what relationship the first experiment bears to the second group, and its results seem to give little information about personality beyond indicating that obsessionals show less tendency than do normals to a gradual modification in the size of the projected image. This seems to be paralleled, however, by the observations made on the tachistoscopic perception of drawings and photographs of faces, that obsessionals tend to give 'all-or-none' reactions rather than having gradually and continuously modified perceptions.

The results obtained with the T.A.T. pictures are more interesting. It does appear that the series of percepts followed somewhat the same line of development with different pictures; for instance, with one subject reports of a woman were consistently replaced at later stages by those of a young child, and later still of an older child. The author claims that these sequences were closely related to particular types of experience in childhood; thus orphaned youths tended to see a 'mother image' in the later stages of the series, where normal controls usually reported a 'father image'. Though case histories and free associations were obtained from many of the subjects, the correspondence with sequences of percepts has not been fully worked out.

It is arguable that these sequential responses may themselves come nearer to imaginative constructions than to true perceptions, though it is suggested that the subjects really thought that they saw what they reported. Whatever the relation to developments in personality, it is doubtful if they have any relevance to other types of perception. Certainly it cannot be concluded that all perception passes through an 'actual-genetic' series of stages which can be 'regarded as a micro-model of the ontogenesis of the personality'. It is possible that the author does not wish to claim as much; but his argument is obscure and hard to follow. Nor is it clarified by the arrangement of the book, which states the conclusions and discussion at the beginning, and describes the experimental methods and results at the end.

However, the method of examining sequential changes in perception appears to be a promising one; and these might well be studied on their merits and without regard to psycho-analytic interpretations.

M. D. VERNON

Experiment in Depth: a Study in the Work of Jung, Eliot and Toynbee. By P. W. MARTIN.

London: Routledge and Kegan Paul. 1955. Pp. 288. 25s.

This is an excellent book, beautiful in the sheer economy with which many profound ideas and feelings are expressed in few words. It is mainly for the thoughtful layman, and for the psychologist who has specialized in fields other than Jungian analysis. Experienced Jungians will find much in it familiar, but there is a new and interesting attempt to explain the difference between men and women, which was never really explicable within the Jungian scheme.

Toynbee's thesis is that in periods of political trouble human beings turn inwards and find a solution in the depths of their own psyches. Martin sees the present world as divided into totalitarian states, which foster in the psyches of their subjects a split whereby people project all the bad on to their enemies and all the good on to themselves and their leaders, and free nations in which people have no sense of purpose, and so make no proper use of their freedom. He thinks it necessary for the citizens of the democracies to regain a sense of purpose by exploring their deep unconscious, a process which Jung has described scientifically and Eliot in poetry. This exploration leads to experience of a religious quality, and Martin thinks there is no ultimate conflict between science and religion, since the nature of such experiences can be verified when different people compare what they have undergone.

Experiment in Depth is well illustrated with dreams, with accounts of waking experiences of the growth of symbols, and with quotations from Eliot's poetry, and from many religious writers, including a number of Quakers. It will puzzle or anger the one-sided thinking type, which means that some psychologists will find it unscientific. It will stir other readers' feelings deeply, and arouse awe and a sense of the numinous by the mere reading of it. It throws unexpected light on familiar things: the present writer had never before realized that the grain of mustard seed and the pearl of great price were Self symbols.

Martin warns against many of the pitfalls in the exploration of the unconscious, and he is a good avoider of pitfalls himself. Although he is at times nostalgic for the Middle Ages, he knows that the Medieval Church was totalitarian. But he has some blind spots. He belongs to the feeling type, and, in a way characteristic of this type, he subtly undermines the religion he appears to be defending; for he writes, in effect, that it does not matter what God is like as long as people have religious experience on the feeling level. He knows nothing of the positive value of Freudian analysis, and nothing about small children.

The repeated cry for a creative 'minority' to save us seems odd to anyone familiar with the fresh and individual way in which every child approaches life, and it is odd to discuss the difference between men and women without once mentioning the difference in our bodies.

Martin recommends that people should explore their own unconscious without the aid of an analyst, by methods which range from meditating on their own dreams to spending a whole night alone out of doors. He thinks the dangers of the procedure must be accepted because of the desperate state the world is in. He does not mention the shortage of analysts, but it has obviously influenced his thought. It is not certain that his diagnosis of the main weakness of the democracies is right. Both militarists and pacifists appeased Hitler, and neither lacked a sense of purpose. However, the exploration of the deep unconscious can remedy other states of mind as well as purposelessness. The real value of *Experiment in Depth* will be known only after the passage of a number of years.

JANE DARROCH

FERENCZI, S. *Final Contributions to the Problems and Methods of Psycho-Analysis*. Edited by MICHAEL BALINT. The International Psycho-Analytical Library, edited by Ernest Jones, No. 48. London: The Hogarth Press and the Institute of Psycho-Analysis. 1955. Pp. 447. 30s. net.

This is the third volume of papers and contributions to psycho-analysis by Ferenczi to be published in the International Psycho-Analytical Library. It contains thirteen papers written between 1926 and 1933, after the publication of the previous volume, seven posthumously published papers, written between 1909 and 1922, notes and fragments written between 1920 and 1932 or undated, and papers, 'varia' and reviews omitted from previous collections and dated between 1908 and 1922.

The main contributions in this volume are important, and it is excellent to have them published in this way. All the material in the volume has been admirably edited and translated, and reads as though English had been Ferenczi's native language. It is very satisfactory to have the date of almost every item and to be able to follow the development of Ferenczi's thoughts in relation to the history of the psycho-analytic movement. The volume contains a bibliography and a 50-page index.

While the interest and importance of most of the papers cannot be doubted, it seems an open question whether even Ferenczi himself would have approved of the inclusion of all the notes and fragments, some of which seem as though they can hardly be intelligible even to a trained psycho-analyst. However, there is some psychological interest in a cross-section of the random thoughts of a great man.

R. W. PICKFORD

Psychoanalysis: Practical and Research Aspects. By WILLI HOFFER. London: Baillière. Tindall and Cox. 1955. Pp. x+102. 27s. 6d.

In 1953 Dr Hoffer was invited to give the Abraham Flexner Lectures at Vanderbilt University. Judging by this volume, containing the substance of his lectures, he has certainly maintained the high standards of the previous holders of this lectureship. Though small in size, it ranks with Freud's *Lay Analysis* as one of the best introductions to psycho-analytic theory. Indeed, Dr Hoffer has laid emphasis on the developments in psycho-analysis since Freud. In this country, any advance in psycho-analytic theory and technique has usually been identified with the work of Melanie Klein and her followers. Yet the contributions of Mrs Klein and Fairbairn pale beside the major contributions to scientific psycho-analysis made by Hartmann, Kris, Loewenstein, Rapaport and others in the United States, and Anna Freud, Mrs Burlingham and Hoffer in England. It seems very likely that when psycho-analysis, experimental psychology and medicine reach a synthesis, their meeting-place will be ego-psychology.

Hoffer introduces his topic by reference to the principles of psycho-analytic training, the concepts of internal conflict and anxiety, and recent contributions to the psycho-analytic investigation of childhood. He continues with a full report of a case of his own illustrating the ego-reactions in cerebral disease, and concludes with a chapter on some problems of ego-psychology. Dr Hoffer does not indulge in flights of fancy, but studiously attempts to define the concepts of modern psycho-analytic theory, and their relation to actual observation. This little volume, which is the most modern exposition of Freudian psycho-analysis, will prove an eye-opener to those who feel that psycho-analysis has parted company with medicine and science. It should prove of particular interest to those who are concerned with problems of the development of memory, thinking and perception. In addition it is admirably suited to the University student who wonders what psycho-analysis is all about.

Any criticism of this book cannot be of the psycho-analysis involved. Hoffer is one of the most eminent of the psycho-analysts trained by Freud, and one can only complain that the book is expensive for its size and that lectures given verbally may not read as easily as they are heard.

JOSEPH SANDLER

The Object Relations Technique. By HERBERT PHILLIPSON. London: Tavistock Publications Ltd. 1955. Pp. x+224. 21s. net; with Test material, 3 guineas.

At first sight Mr Phillipson's contribution to the published range of projective techniques would appear to be little more than yet another variant of T.A.T. It has, however—in contrast to both T.A.T. and Rorschach, which are somewhat loosely based on an eclectic depth psychology—the distinguishing feature that it has been designed and developed as an adjunct to therapy carried out within the framework of a single theory of the development of personality—the object-relations theory of which Klein and Fairbairn are the best-known proponents. That being so, it is appropriate that the central chapter (extending to more than half) of the book should consist of illustrative case material. Of this chapter half, again, is devoted to detailed study of a single case, with very full interpretations of all aspects of the subject's response.

The test material consists of twelve pictures and (as in T.A.T.) a blank card. There are three series of pictures, distinguished by the style of drawing: *A*, in light charcoal shading, misty in quality; *B*, in heavy shading approaching a silhouette technique; *C*, with colour introduced, and more definite detail. In each series one picture presents a one-person situation, one a two-person, another a three-person and the fourth a group situation. The *A* series, it is claimed, 'will stimulate primitive dependent needs and the consequent anxieties'; in the *B* series 'the emphasis (is) on phantasy relation with threatening and uncompromising objects'; the *C* series is intended 'to present a world rich in opportunity and challenge'. How far this attempt to systematize stimulus material has been successful is not clear from the normative data provided. Thus, figures derived from two samples—of fifty clinic out-patients and of forty normal adolescent girls—show that direct references to colour are made in only about one-third of responses to any one picture; evidence on the relative specific functions of the *A* and *B* series, as defined above, is also lacking. The present reviewer has also formed the impression that the extreme ambiguity of, particularly, the *A* pictures may produce a Rorschach-type attitude (puzzling over detail etc.) which tends to inhibit both the production of a 'story' and the identification phenomenon stressed by Henry Murray. In view of the underlying theory this may not be important in the clinical situation proper, but if, as the flap description suggests, the material may also be used 'outside the clinical field, in social research and in personnel selection in industry and elsewhere' it may be that avoidance of structuration has been carried to excess.

One will await with interest further reports of the use of the technique (as promised, e.g. in articles listed as 'In preparation'). Perhaps it will not be regarded as too uncharitable also to hope eventually for a clearer account of its 'quantitative possibilities' to which Dr J. D. Sutherland refers in his Foreword. The author must, however, be congratulated on the degree of success he has achieved in his main purpose—the linking of the projective hypothesis with current thought in perception theory, and the application of both to a coherent system of personality dynamics.

BORIS SEMEONOFF

Recent Developments in Psychosomatic Medicine. Edited by ERIC D. WITTKOWER and R. A. CLEGHORN. London: Sir Isaac Pitman and Sons Ltd. 1954. Pp. xvi+495. 50s. net.

As the Editors say of this book 'it is not meant to replace the admirable book *Psychosomatic Medicine* by Weiss and English. It is meant to serve the purpose stated in its title—to describe modern developments in psychosomatic medicine.' It is not as encyclopaedic as the new fourth edition of Dunbar, but it is more easily read. Twenty-nine authors contribute twenty-five chapters. All except three authors have medical qualifications and most have psychiatric appointments. Several are psycho-analysts.

Coverage is not limited to areas in which psychological factors have, in the past, been presumed of etiological significance. Medicine, 'in order to be good medicine must be psychosomatic irrespective of the extent to which psychological factors are of etiological significance'. Hence chapters on pulmonary tuberculosis, venereal disease and disablement have been included.

The book is in two parts—general and specific. The eight chapters in the former are likely to be of value to all psychologists whose work brings them into the medical situation, especially Chapter 1, 'Basic

Concepts in Psychosomatic Medicine'; Chapter II, 'The Problems of Specificity in the Psychosomatic Process'; and the two chapters on research by S. G. Wolf and by R. B. Malmo.

In Chapter I, Macleod, Wittkower and Margolin present for comparison the underlying conceptual models on which the current theories of psychosomatic medicine are based.

Kubie's Chapter II is concerned with one of the basic questions to be faced by psychologist and doctor in this area—the problem of specificity. He ends with a question, 'What is there which is of a peculiar, or special or different or specific nature about the regression and dissociative processes which result in physiological disturbances?' Kubie refers to two long-term projects which will help to settle some of these questions; one in which investigators recorded physiological variations in children, who will be followed through coming years for their subsequent somatic and psychological histories, and another at Johns Hopkins Medical School where all medical students are now subjected to intensive psychological scrutiny, and through the coming years these doctors-to-be will send back to the study-group physiological data on their own blood-pressure, pulse rates, etc.

Malmo's Chapter V on 'Research: Experimental and Theoretical Aspects', argues for the importance of theory construction in this area and for the desirability of clinician and experimentalist working together. He outlines the value of electromyographic techniques when combined with clinical research.

MacLean's Chapter VI, 'Studies on Limbic Systems ("Visceral Brain") and their Bearing on Psychosomatic Problems', brings together material from comparative neurology and recent work on the frontotemporal region in relation to the subjective aspects of certain syndromes: '...one can make the general inference that the frontotemporal region is concerned with the oral activities of the animal as they pertain to feeding and to the vocalisation, attack, and defense involved in obtaining food. In an effort to go further and to obtain insights into the subjective functions of this part of the brain one derives a wealth of material from a clinical study of psychomotor epilepsy.' Kubie, in Chapter VII, groups the body involvements in psychological conflicts under four categories and draws on the work of Penfield and others. 'Consequently we are justified in saying that the temporal lobe, with its deeper primitive connexions, is a mechanism for the co-ordination of all the data which link us to the world of experience, both external and internal... It is the area of the brain which deserves to be viewed as the organ which is essential for psychosomatic relations.' E. L. Margetts contributes a valuable chapter, 'Historical Notes on Psychosomatic Medicine', in which he sums up the literature on the body-mind problem in medicine from ancient times to the present day.

Part II, 'Specific', will be of interest to the psychiatrist, but in both it and the preceding part he will need, if not some sympathy with dynamic psychiatry, at least a fairly advanced knowledge of its terminology. The psychologist will find most of these chapters useful as a beginning when asked to co-operate with the psychiatrist on a psychosomatic problem. There are chapters on diabetes mellitus; rheumatoid arthritis; skin diseases; cardiovascular disorders, heart disease and hypertension; peptic ulcer; non-specific ulcerative colitis; and psychosomatic disease due to battle stress.

The reviewer found the following particularly valuable: (a) Cleghorn on 'The Interplay between Endocrine and Psychological Dysfunction', (b) D. O'Neill on 'Psychological Aspects of Gynecology and Obstetrics', with its welcome caution which might have come earlier in the book—'Because analysis of mental content has illuminated much that was hidden in the causation of disease, it is tempting to suppose that this way lies the answer to the question [Why a person gets ill in a particular way]. But the determinants of the 'focus' of illness can well be of a different order and there is room for research on this theme. The prime need, in gynecology and obstetrics, as elsewhere in medicine, is for the hard currency of fact'; and (c) the chapter by the two psychologists on disablement.

This volume adds a little to the impression that psychosomatic medicine, while preserving the rather unfortunate adjective is 'becoming incorporated into the general body of medical knowledge', passing from the stage where it was largely propaganda, and statements about coincidental phenomena, to a period of combined research by clinician and experimentalist into the relevance of psychological phenomena in the etiology of disease.

JOHN C. KENNA

The Mental Hospital. A Study of Institutional Participation in the Treatment of Psychiatric Illness. By ALFRED H. STANTON and MORRIS S. SCHWARTZ. London: Tavistock Publications Ltd. 1954. Pp. xx+492. 35s.

This is a highly interesting account by a psychiatrist and a sociologist of their examination of the community structure of a rather unusual hospital. Formal and informal activities among patients, between patients and staff and among staff are reported and discussed. Much of the book is given over to detailed

reports and accounts of what goes on in administrative psychiatry rather than in the psychotherapeutic attack on psychoses. This focus is epitomized in the catch-phrase 'the other 23 hours', in which treatment can be effectively sought, over and above the formal sessions between patient and doctor.

The centre of the investigation is a disturbed ward of fifteen chronic patients, mainly suffering from schizophrenia, but the interaction of the whole hospital community is highlighted. The enormous difficulties of personal and group attitudes of communication and relationship and of morale, aroused in trying to develop the positive therapeutic atmosphere in social interaction, are faithfully reported. The reader who has experienced the problems of the effects of institutionalization superimposed upon the effects of psychoses will readily understand the setting of the investigation and the significance of the difficulties, misunderstandings and disagreements which are revealed. It may well be that the more general reader will be merely puzzled that such things have not been obvious before, but he can find as much of interest in the problems of method as the clinical psychologist can find in the results.

R. M. MOWBRAY

Textbook of Occupational Therapy. By E. N. M. O'SULLIVAN. London: H. K. Lewis. 1955. Pp. x+319. 21s.

There is no doubt that a comprehensive text-book on occupational therapy is badly needed, and Dr O'Sullivan's book will go some way to meeting this need. Apart from the chapter on the history of occupational therapy, which is of general interest, it is difficult to decide precisely for whom the book has been written. It is too elementary for the trained worker and too detailed for the administrator or the general reader. If it is intended for students, the absence of any illustrations whatever is a grave handicap, and some of the reproductions of report forms are very poor.

The author makes a very proper plea for adequate occupational therapy accommodation in the hospital, but his sketch plans of an ideal occupational therapy department, where each craft is housed in a separate room, would seem to vitiate one of the great advantages of this form of therapy, namely, the possibilities it affords for the formation of groups. Patients in such a building as Dr O'Sullivan describes would have to be arbitrarily grouped according to craft and not therapeutic needs.

There is a large bibliography in which books written twenty years ago or more predominate. In the psychology section, for example, nothing later than McDougall and the 1935 edition of Boring, Langfeld and Weld is quoted.

RALPH HETHERINGTON

Maladjusted Children. By CHARLES L. C. BURNS. London: Hollis and Carter. Pp. x+81. 6s. net.

Dr Burns, the Senior Psychiatrist to the Birmingham Child Guidance Service, has written a simple and interesting account of the main problems, aspects and methods of dealing with children who are maladjusted. The book includes nine short chapters covering the broad types of maladjustment, clinical methods of child guidance, aspects of natural development, problems of sex in childhood, delinquency, adolescence, discipline and the relationship of education to mental health. It is clearly written, without undue technicalities, and is eclectic in its approach to psychotherapy, with a generally Catholic religious outlook. For parents and teachers it would be admirable.

R. W. PICKFORD

Psychopathology and Education of the Brain-Injured Child. Volume II. By ALFRED A. STRAUSS and NEWELL C. KEPHART. New York and London: Grune and Stratton Inc. x+266 pages. 6 dollars.

The original book of this title, now to be regarded as the first volume of a projected series, was published in 1947. It is well known for the authors' views on the diagnosis and treatment of some types of mental defect or abnormality encountered in children, which derive, as regards theory, chiefly from the Gestalt school. The present book is not merely a revised edition, but embodies subsequent thought and clinical experience without going over the former ground again. It contains much interesting and valuable discussion in its four central chapters, entitled 'Psychopathology of Perception', of 'Language' (treatment of aphasia is summary and reserved for a further book), of 'Concept Formation', and of 'Behaviour'. No new theoretical views are propounded, but an extension into the field of the brain-injured child with 'normalcy

in I.Q.', but behavioural deviations, is made. The first chapter deals with the anatomy and physiology of the central nervous system, but would be irritating to a serious student of these subjects. The text attempts to cover too much detail for its eighteen pages, most of which are, in any case, occupied by drawings which are also overloaded with detailed legends. One cannot but regret the supersession of the simple, but perfectly adequate, chapter on these topics in the original book. In general, it must be said, the illustrations serve the book poorly. They are crude in conception, too expansive and numerous for their quality, and, on occasion, e.g. Fig. 26, p. 130, make nonsense of the text. They are, however, beautifully printed on excellent paper.

Among the final chapters and appendices should be noticed the contribution by Samuel Goldenberg on 'Testing the Brain-Injured Child with Normal I.Q.', which considers controlled experimental evidence, rather than the fruits of clinical experience and cogitation, and gives a clear exposition of the scientific weaknesses of the main diagnostic distinctions on present evidence. This is, of course, the lack of a satisfactory criterion of 'brain damage', which is independent of the psychological tests supposed to show it, and against which they can be validated. The standard neurological indicators are expressly rejected in the dubious cases, on quite reasonable grounds; but it leaves an awkward gap in the logical sequence, which is liable to a 'closure' effect and to achieve a nice circular symmetry. Until the association of psychological and neuropathological findings has been demonstrated in the same way as for the neurological ones, the value of the test findings for this purpose remains dubious. The 'exogenous-endogenous' distinction may still, however, be perfectly useful empirically for educational purposes, although some more evidence on this point, too, would be welcome. Dr Strauss is well aware of the theoretical shortcomings of his thesis, and makes a good case for his procedure; but it is a little disingenuous to say, 'This would appear to be the same type of reasoning that is used in the mathematical problem of factor analysis'.

J. A. L. NAUGHTON

Studies in Education: 7. The Bearings of Recent Advances in Psychology on Educational Problems. University of London Institute of Education: Evans Bros. 1955. Pp. viii + 215. 7s.

Following on the last volume in this series, the London Institute of Education has again printed a number of lectures by different authorities centred round a single theme. Appropriately enough for the seventh volume of this series, there are seven lectures. We start with a paper by Prof. Rex Knight on 'Children's Needs and Interests'—contemporary psychological theories of motivation. Then come papers by Dr C. M. Fleming on the 'Child within the Group'—the bearings of field theory and sociometry on children's classroom behaviour; Mr Ben Morris on 'Mental Health in the Classroom'—the teacher's personality and problems of children's adjustment; Mr H. A. T. Child on the 'Backward Child'—the incidence, causes and treatment of backwardness; Dr W. D. Wall on 'Teaching Methods'—psychological studies of the curriculum and of classroom teaching; Prof. E. W. Russell on 'How Children Learn'—contemporary theories of learning; and finally Prof. P. E. Vernon on the 'Assessment of Children'—recent trends in mental measurement and statistical analysis.

Of the value of this book there can be little doubt. For the teacher and educationist, too busy with the practice of their craft to hope to keep in touch with the growing edge of psychological theory and research it covers a reasonably high proportion of this most adequately. The technique of the spoken lecture and the written paper vary somewhat, and examples are not unknown of successful lectures which in print have been less than satisfying. This difficulty is not obtrusive here; in general, the book reads well and only occasionally is one reminded of the lectern. Indeed, in one respect (that of length) the reader is forced to wonder how *all* the contributions could have been single lectures. The four University contributors are, no doubt, showing their occupational skill when they produce chapters which are almost identical in length. If one assumes that they each spoke for forty minutes, then it appears that Mr Child and Dr Wall took just over an hour, and that Mr Morris was only just coming to his peroration at the end of two hours. No doubt Mr Morris's ample footnotes (and footnotes to footnotes) account for part of this, but hardly for all.

It is likely that the chapters on backwardness and on teaching method will have the greatest initial impact on the practising teacher, since they are most obviously orientated towards immediate practice as well as being supplied with ample references to research work on practical problems. But Russell's attempt to lead the reader through the labyrinths of contemporary learning theory, supplemented by Knight's analysis of motivation (and Fleming's elaboration of field theory), are likely to be welcomed by

any to whom the diversity of theory has been a challenge too formidable to accept. The job of structuring the field for the benefit of those who must eventually *apply* theory is not attempted often enough. We must be grateful for this, not only to Knight and Russell, but also to Vernon. The final chapter, although ostensibly covering mental measurement in general, deals mainly with the concept of intelligence. It gives, in Prof. Vernon's inimitable way, the first account accessible to the general reader of his view of intelligence and its bearing on educational selection. The postulation of 'intelligences' A, B and C, derived to a considerable extent from Hebb and Piaget, is beautifully explained, and the implications are illustrated by reference to beliefs commonly held by teachers. These are taken from a questionnaire answered by a group of London teachers, the results of which show very clearly the confusion which exists in this field. It does not seem unlikely that some teachers hold a set of opinions which are mutually compatible. This is not surprising after the confused controversies in the educational press during the post-war years. This chapter is a bold attempt to produce order out of chaos by offering a *via media* based on analysis of the known facts. No doubt it will be attacked from both flanks, but to the teacher it may well seem the most significant step forward in this area of educational psychology since Spearman's theory of *g*.

It is, perhaps, unfair to criticize a book of this kind for what it does not contain. But in a publication emanating from an Area Training Organization it is a pity that teacher-training does not receive the emphasis it needs. It is mentioned incidentally—for example, in Morris's long chapter—but a fuller and more eclectic treatment would have enriched the book. But in spite of this, we must be grateful to the London Institute of Education for a volume whose usefulness for many teachers will far outlast its paper covers.

STEPHEN WISEMAN

Psychology in Education. By J. B. STROUD. London: Longmans Green and Co. 1955.
Pp. vi+664. 32s. 6d. net.

This is a straight reprint of a book first published in the United States in January 1946. Internal evidence suggests that most of the book was written in the late 1930's, with certain later additions. The bulk of the references given are to researches prior to 1940, and no reference is made to work later than 1944. The book is now published in the United Kingdom in 1955.

This means that all post-war educational psychology is omitted, and some of these omissions are important. The opening chapter, which is quite an extensive review of the sociological aspects of education, performs much more general and discursive than a corresponding chapter written today would be, as it lacks the body of information accumulated in the past ten years in that field. Similarly, the section on Piaget contains no reference to Piaget.

This is not to say, however, that the book is wholly useless. It is, indeed, a rather odd mixture; nearly everything has been put into it. An objective, scientifically based chapter on 'Individual Differences' is preceded by a rather old-fashioned one on the 'Foundations of Learning', though in the latter the use of the historical approach is one which could be more frequently used in such books. Scientific facts rub cheeks with educational moralizings. There is, in short, a mixture of good and bad, and though the volume is not one which can be wholly recommended there are several chapters in it which are soundly based and could be studied with profit.

JAMES MAXWELL

Statistics in Education. By MERLE W. TATE. New York and London: Macmillan. 1955.
Pp. x+597. 42s.

The intention of this book, as described by Prof. Tate in the preface, is to serve education students taking one- or two-semester course. The approach to the subject is mainly 'through common sense and arithmetic'. The author is obviously aware that many students of education are not mathematicians. Thus many of the algebraic formulae are merely stated, though some are derived in the text and in an appendix, after the manner of Guilford.

Yet this is more than a mere cookery book. Its author respects the intellectual quality of the reader. From her introduction, which she opens with an epistemological discussion, to her final chapter on 'Statistical Inference' (in the reviewer's opinion the best in the book), Prof. Tate demands from her readers some interest in underlying philosophical problems and appreciation of logic. It is not that she goes deeply into these issues; rather she whets the appetite, and in the references following each chapter indicates where the solid fare is to be found.

In one respect the reviewer was disappointed. The treatment of analysis of variance, while covering rather simple examples of one- and two-way classification, the test for linearity or regression, and with some mention of multiple correlation, does not introduce the student adequately to the wider possibilities of experimental design.

In its coverage of statistical techniques the book is conventional, with numerous illustrative examples and diagrams. Factorial methods of analysis are omitted. With the exception of analysis of variance the treatment is rarely superficial. The student is made to realize that means other than the arithmetic exist and have their uses; that there is some point in squaring deviations from the arithmetic mean rather than some other origin; that the formula for the correlation coefficient makes sense; that any attempt, on statistical grounds or otherwise, to foretell whether a particular child will succeed or fail, is nugatory; and that confidence limits require careful interpretation.

An interesting feature is the description of experimental studies of the distribution of such statistical artefacts as t , F and X^2 , after the manner of Snedecor (whose *Statistical Methods* this book in some ways resembles). For the non-mathematical reader, such studies should do much to imbue with meaning these statistical commonplaces, so often accepted and employed uncritically.

A. E. G. PILLNER

Crime and the Services. By JOHN SPENCER. London: Routledge and Kegan Paul. 1954.
Pp. xii + 306. 28s.

This is an account of a research into the effects of Service life on criminal behaviour. Of the many aspects of this problem, the investigator sets out to concentrate on two: the delinquent in the Services and the 'Ex-Serviceman' in prisons and Borstal.

The research was conceived not only as a testing out of criminological theories in a particular field setting but as a practical contribution to the prevention of delinquency, both military and civil, in the Services. This latter involved an investigation of the methods of correction employed in the treatment of Service offenders. Questions of screening, selection, the discharge of criminals from the Services and the criminogenic influence of the resettlement period for all the Ex-Servicemen are discussed as specially relevant.

Part I of the book presents an analysis of the problems of adjustment to Service life in the last war, an account of Service crime and criminals and the techniques used for their reformation. Part II begins by examining the problem of the Ex-Serviceman's readjustment in civil life and includes a chapter on the special difficulties of the ex-P.O.W. Up to this point Dr Spencer's method is the usual one of collecting and collating information and statistics, together with a critical survey of the existing literature. This is done comprehensively and assiduously even to the point of being over-written with an excess of references and footnotes of every conceivable relevance.

The remainder of Part II consists of a field investigation of three groups of Ex-Service criminals (100 prisoners at Dartmoor, a similar number at Maidstone and 82 lads undergoing training at Sherwood and Gringley Borstals). There is a further study of the after-care records of 200 discharges from various prisons. By comparison with the previous section this part of the book is much less impressive, and one can easily find fault with the sampling, the general presentation of the material, the thinness of some of the case histories, the naïve personality typing and so on. Dr Spencer is acutely aware of much of this and attempts to justify it by reference to such difficulties as the poverty of prison records. One would regard such arguments as double-edged.

Part III begins with a critical assessment of the effect of institutional life in the penal establishments referred to above in terms of the inadequacy of ordinary reformative methods for meeting the needs of the 'chronic delinquent'. Here especially one detects an expression of opinion arising out of Dr Spencer's field experience but going far beyond his presented data. His final conclusions, however, are cautious and balanced. He rejects equally firmly the hypothesis that Service life has itself a criminogenic influence on its participants and the claim that it is a cure for delinquency. He would, however, admit that Military Service, and particularly its wider social repercussions, may have unfavourable effects 'for certain types of personality in certain situations'. He emphasizes especially the effects of separation from family life and the significance of the demobilization period.

Finally, there is a useful Appendix on Methodology which should be read by all future research workers in the penal field and, for their sins, by those who are concerned with the compiling of official records.

Dr Spencer claims that his research had to be carried out 'piecemeal rather than in a logically coherent form'. To this should be added his admission that 'the by products of research appeared to the writer to

of equal interest to [its] main object'. This is only too clear from the loose organization of the book as a whole. One would suspect that Dr Spencer was faced with the problem of how to achieve a concise presentation of his material and yet satisfy his need to include every detail of his field experience and its impact upon him. Perhaps a better solution would have been for him to have written two books, one a more rigorously integrated presentation of his research findings and the other a more frankly personal account of the effect of his investigations on his attitudes and opinions.

Those, however, who can cope with the mild irritation which such defects may arouse and who will take the trouble to follow up Dr Spencer's many suggestive leads will finish up with a much broader understanding of delinquency in general and its treatment possibilities. From this latter point of view especially this book is certainly to be recommended.

R. L. MORRISON

Survey Report on Human Factors in Undersea Warfare (1949) and *Supplement* (1954).

Prepared by the PANEL ON PSYCHOLOGY AND PHYSIOLOGY, Washington, D.C. Committee on Undersea Warfare, National Research Council. Pp. x+541. Supplement, pp. i+92. No price given.

Approximately three-quarters of this book is devoted to the methods, findings, and some of the problems of what has now come to be called engineering psychology. Although special reference is made to conditions in submarines, the majority of the material is quite general in its applications, and about half of it is most useful.

The topics discussed include the psychophysics of vision, the reading of printed materials, the design of instruments for displaying information, fundamental considerations in designing controls, the design of working panels containing displays and controls, systems research, the effect of noise and distortion upon voice communication, lighting, and the shortcomings of current placement and training procedures. Among more physiological topics are temperature regulation, the sleep-wakefulness cycle, and motion sickness. A section is also included on stress, and the conditions relating to group effectiveness. The chapters on the night lookout, the sonar operator, diet, and possible methods of visual communication, may be of less interest to the general reader.

It is now nearly seven years since the book was first published, although the supplement has only recently become freely available. No mention is therefore made of more recent experimental work, such as that on visual watch-keeping and multi-channel listening, to name two examples. However, throughout the book there is a strong emphasis on the shortcomings of previous research, which was mainly conducted under the stress of wartime conditions. This emphasis is refreshing, and is still exceedingly relevant. In addition, the directions in which research could usefully proceed are described in some detail, and have not changed much in the last seven years.

E. C. POULTON

The Non-Human Primates and Human Evolution. Arranged by JAMES A. GAVAN. Detroit: Wayne University Press. 1955. Pp. 134. \$3.50.

The papers in this symposium were presented at a 1953 meeting of the American Association for the Advancement of Science, and they come from anthropologists, psychologists, palaeontologists and comparative anatomists. The book is dedicated to the anthropologist Hooton who took part in the symposium, but died soon afterwards. There is no doubt that his contribution is by far the most provocative, and suggestive of future research. He makes a strong case for genetic studies on lower primates, and for more studies of infra-human primate societies which, he considers, are far more fruitful than 'the stuff on present-day savages written by anthropologists'. The book as a whole is aimed at explanation rather than description. The lucid chapter by the comparative anatomist, D. Dwight Davis, is perhaps the most successful in this respect.

There are three chapters by psychologists: Carpenter provides a useful summary of infra-human primate social grouping, better described as infra-human sociology; Nissen's chapter is concerned with explaining to non-psychologists the work of the comparative psychologists; and the chapter by Mr and Mrs Hayes describes the 'cultural capacity' of a chimpanzee, Viki, reared for six years in their home. This last is interesting in that several of the author's remarks, for instance on thumb opposition, posture and the use of tools, and brain size, are unorthodox.

It is stated that the papers have already appeared in *Human Biology*, September 1954.

BRIAN FOSS

The Human Brain. By JOHN PFEIFFER. Pp. vii+273. London: Victor Gollancz Ltd. 1955. 16s.

This book is written by the Science Director of the Columbia Broadcasting System and President-elect of the Science Writers Association. It is essentially a work of popular exposition. Its material is presented with brisk assurance, and, in general, with clarity, but necessarily at second-hand for its main facts. No extensive scientific vocabulary is required, but some familiarity with North American colloquial speech would be a help. 'Jibe', for instance, appears in an unfamiliar sense. Well-known authorities on the various topics have been consulted, in many cases personally. Amongst them one pauses a little before recognizing 'Edgar D. Adrian'. Maximal use is made of the personal anecdote, the abstracted case history, the actuality description, and reference to matters of current general interest, and prejudice. These last are naturally better adapted to an American public than a British, and emphasize the specificity to which the popular approach is liable, in contrast to more austere methods.

All these devices for sustaining the interest of the uninformed are put to good didactic use, and an eminently readable and informative book has been produced.

The chief reservation in one's praise would be some anxiety lest the production of a simplified account of experimental work in progress should leave an unjustified impression of certainty in the ordinary reader; but this risk is inevitable if the doings of the experts are to be interpreted to the general public, and is probably preferable to total ignorance.

The author is least happy in the more psychological sections. 'Memory and the Higher Faculties' gets sixteen pages and rather confused treatment. The mental changes with ageing are presented in a hesitating and inconclusive way. This contrast in lucidity with the rest of the book is presumably the fault of the primary authorities; or perhaps it would be fairer to say that it indicates that this field is the least ready for dogmatic treatment.

J. A. L. NAUGHTON

Management of Addictions. Edited by E. PODOLSKY. New York: Philosophical Library Inc. 1955. Pp. 413. \$7.50.

This book will be found most useful by medical practitioners having to deal with drug addicts on whom previous methods of therapy have failed. The greater part deals with the treatment of alcohol addiction and some thirty authors with first-hand experience describe the wide range of methods available and attempt to evaluate their results. One is struck by the preponderance of therapies involving hormones and drugs, while the once popular use of vitamins is barely mentioned. Among other drugs of addiction discussed barbiturates and 'benzedrine' are included side by side with the more spectacular morphine and hashish.

Psychologists will be interested in the relative contributions of physiological and psychological factors to the development of addictions—a problem which is approached from various points of view including the psycho-analytic—and in the accounts of applications of the more psychological methods of treatment. Among these are group psychotherapy, hypnotic suggestion, and various forms of 'conditioned reflex treatment' in which something nasty is associated with the addicting agent. A good deal of case history material is given, much of it, however, too superficial to throw useful light on the kind of personality which prefers drug addiction to other means of gratification. What, for example, is one to make of isolated cases like that of the patient reported to have turned alcoholic on discovering that he had lost the ability to 'reach to ecstasy' while playing his saxophone?

The book is in the form of thirty-five separate essays of somewhat uneven quality and with inevitably a certain amount of overlap between them, though most of them are to be commended for ending with a short summary and a list of references. A subject index would have made the contents still more accessible.

HANNAH STEINBERG